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On the Occasion of his Seventieth Birthday

Edited by

George Cardona
and
Norman H. Zide

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FOREWORD

We are happy to pay homage to Henry Hoenigswald with this volume of studies by colleagues and friends, several of whom have had the privilege of studying with him. It is fitting that scholars from all over the world should join in expressing their esteem for a colleague whose many varied and fundamental contributions to historical linguistics and the history of linguistics are so widely known and appreciated:

vidvān sarvatra pūjyate

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Binary and multiple oppositions in the history of Indo-European

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1. This paper is written on the strength of a series of ideas which I have attempted to justify elsewhere and which I am here obliged to take simply as a starting-point.[1]

In fact it deals with the idea that the development of Indo-European took place in three stages, which may be defined schematically as follows:

Stage 1: Pre-flexional Indo-European or Proto-Indo-European (IE 1)

This functioned on the basis of root-words, either nominal-verbal or pronominal-adverbial ones, which determine each other to make up syntagms and sentences through word-order, accent placing and certain enlargements, albeit without proper inflexion and without the later categories of Indo-European having yet arisen.

Stage 2: Monothematic Indo-Eurpean (IE 2)

This phase, preserved in Anatolian (which was left isolated to the south of the Caucasus) and in archaisms in other languages, already has inflexion although always on one stem. In the noun, it opposes singular and plural, animate and inanimate, and has a case system; in the pronoun, it opposes persons; in the verb, singular and plural, three persons, indicative and imperative, present and preterite, active and middle voice. These oppositions are basically expressed by means of oppositions between series of endings. There is the beginning of stem opposition: basic and derived verbs (causative, iterative, etc.); sg. and pl. Nom.-Acc.-Voc. n. (*-om/-ā, -ă*); heteroclitic nouns.

Stage 3: Polythematic Indo-European (IE 3)

The former oppositions of categories are maintained in this phase, but some of them are extended: that of gender is now between masculine, feminine and neuter, that of number between singular, plural and dual (but not always), that of tense between present, preterite and (at times) future, that of mood between indicative, imperative, optative and (in certain languages) subjunctive, that of voice between active, middle and (at times, too) passive. These extensions now need stem oppositions. And the same goes for other new oppositions: that of the degrees of comparison in the adjective and that of the aspects in the verb. An old opposition, that of present and preterite, now not only opposes endings but also stems. This type of Indo-European is the one which is the object of traditional description.

According to this conception, there has been progressive complication of the oppositive systems; on the other hand, there have been implications between them, that is, one form

may intervene in more than one system of oppositions (gender, number and case; tense, aspect, voice, number and person). It is, moreover, curious that all the oppositions in Indo-European 2 are binary, with the exception of those of person and case; all are basically carried out with the help of endings, although there are also oppositions between lexical items (nouns which are only neuter or only animate, for example), and, to a minimal extent, between stems (in the above-mentioned cases). In Indo-European 3, on the other hand, although it still preserves the old formal resources of the oppositions, the new resource of stem opposition prevails, either by serving the old oppositions or by extending them or by creating other new ones.

An opposition, in fact, may make use of diverse formal resources, according to its antiquity or the category or function it refers to, and may use one or another or a combination of both.

The remarkable thing, however, is that at the time when the grammatical resources used (the lexical ones existed alongside these) were endings, the oppositions were binary, with one or two exceptions; and when these resources were complemented by other grammatical ones, based on the opposition of stems, they became multiple, being generally ternary. The growth of Indo-European morphology was therefore thus tied up with the above mentioned formal resources and obviously had certain repercussions at the level of content. Yet these repercussions may be summed up as the passing from binary oppositions to ternary ones or even to those with more terms.

Therefore it is a matter of explaining this evolution and the reasons for it. It also means explaining the great exception which existed: the case system which in Hittite has seven terms and in other languages a variable number of them, e.g. a maximum of eight, as is known, in Indo-Iranian.

I believe that a further argument should be added to those I have already given in the above-mentioned publications, in favour of the hypothesis that the binary oppositions are the more archaic and that the grammatical resource based on the opposition of endings is also the most archaic. This argument would be of a general type: grammatical oppositions generally begin as binary through the opposition of two terms (two words, two endings or various grammatical elements). It is clear that the opposition of two words is not yet a grammatical fact; neither are the other oppositions when they are isolated. A series of opposed terms are needed: **pətēr/*mātēr, *bhrātēr/*su̯ēsōr . . . ; -ti/-t, -si/-s; *line k^u̯ti/ *leloik^u̯e, *k^u̯r̥neuti/*k^u̯ek^u̯ore . . .*

On the other hand, the last examples indicate that in the oppositions expressed by grammatical elements one must count upon allomorphism, as one must likewise count upon defectiveness (**u̯oide*, without a present), syncretism, amalgams, neutralizations and other irregularities. As I have explained elsewhere[2], these irregularities shed light upon the invariably secondary nature of the oppositions of inflexional Indo-European (2 and 3).

If this is so, it would appear that the multiple oppositions must forcibly have originated in an even more secondary way: through the split of one term of a binary opposition (the animate split into masculine and feminine); through the addition of a new term which opposes one of the basic binary opposition; or through combinations within a complex system of independent binary systems.

Before going further into this subject, however, I have a few things to say on the origin of oppositions in general: that is, in fact, on the origin of the binary systems of oppositions

which are the basic ones. I believe that this has been acknowledged whenever a diachronic criterion has been applied which was at the same time structural, in order to explain how oppositions appear in one stage of a language which did not exist in a former one; and how formal elements which could not express oppositions which did not yet exist in the former stage could then express them in the later stage. I refer briefly to papers by Kuryłowicz[3] and others of mine[4] in which we refer to precedents of these ideas in A. Martinet and others.

Very briefly:

a) A lexical, or lexico-grammatical item may be opposed as a negative term to the same item when a second element is added to it, thus becoming the positive term. Thus, a verbal stem followed by elements such as *-m, -s, -t, -nt* does not indicate tense; but if this complex *T-m (T-s, T-t, T-nt)* is opposed to another T-*m-i* (T-*s-i*, T-*t-i*, T-*nt-i*) in which *-i* signals 'present', it comes to mean 'preterite' through opposition.

b) Certain elements which originally had no meaning of their own acquired it in certain contexts. For example, *-ā* and *-ē* are originally radical elements (in disyllabic roots); but they are at times defined as characteristics of the indicative (as against other of the subjunctive), at others, they indicate the opposite; or else: *-ā* signals the indicative and *-ē* the subjunctive (Lat. *amās/amēs*). As is today acknowledged, the same goes for the case of the thematic vowel which marks indicative as against the long vowel of the subjunctive or subjunctive as against athematic forms of the indicative, etc. As soon as one of these elements, accompanying a verb in an indicative context, is interpreted as expressing the indicative, it is then used to this end in other different verbs; and the same occurs with the subjunctive. Thus, Brugmann has already explained the conversion into a feminine marker of an *-ā* which was originally radical (in $*g^{\mu O}nā$).

Naturally, the signifiers are later extended and complicated, but the basic pattern is the following: there is always an underlying opposition between two terms, or two series of words.

2. In the second part of this paper, I shall now examine the inventory of binary oppositions in Indo-European, together with their means of expression: I shall give a few indications as to how they were used for the creation of these oppositions. Then I shall examine the exceptions existing within Indo-European 2: ternary systems or those with more terms. This will clear the way for the last part of the paper: the systems of multiple oppositions (through extension of the binary ones or new creations) in Indo-European 3.

The binary oppositions of Indo-European 2 preserved in Anatolian are as follows: in the noun: animate/inanimate; in the verb: indicative/imperative, present/preterite and active/middle voice; in one and the other: singular/plural.

Of these oppositions, that of gender (animate/inanimate) and that of voice (active/middle) have a lexical basis although this is lost in the gender of adjectives, which have "motion". To begin with the former, nouns are by definition 'tantum' forms (either only animate or only inanimate); the concept of defectiveness is habitual, although it may at times be lacking (cf. doubtlessly archaic oppositions of the type tree names in *-os*/fruit names in *-om*, the types represented by Gr. γῆ/πέδον, Lat. *terra/tellus*, O.I. *agnis*/Hitt. *paḫḫur*, Lat. *aqua*/Gr. ὕδωρ). As is well-known, to the lexical item another grammatical one is added: animates with different Nom., Acc. and Voc./inanimates with syncretic Nom.-Acc.-Voc.[5]. The theo-

ry I have expounded in other papers on the origin of the opposition is the following: a sub-class of nouns incapable of performing the function of subject and the impressive-expressive one were opposed to another sub-class of nouns capable of both functions: both types also performed that of object (cf. Adrados 1975: 398 ff). The nouns of the first and second class were organized into two series, endowed with suitable grammatical markers (although the inanimates could also later function as Nominative and Vocative, they extended the old Accusative form to these uses, whence the syncretism).

Thus, a binary lexical opposition was extended to two lexical series and was grammaticalized with one positive and one negative term. Something similar may be said of the voice opposition: there are verbs which are only active, others with different roots which are only middle voiced: they are the lexical basis of an opposition which was likewise grammaticalized. In this case there was systematic opposition of two series of endings (in gender, this occurred in Nominative and Vocative but not in Accusative). Verbal forms with *-e/o* stem or with *-e/o* ending which could appear together with verbal forms of the middle voice in themselves, spread as carriers of this voice; on the contrary, radical verbs without *-e/o* or with endings not followed by *-e/o*, remained characterized as of the active voice. Traces of the secondary nature of the opposition are, among others: the fact of their being at times connected to lexical items still; the fact that stems with *-e/o* or endings with *-e/o* may at times be of the active voice or indifferent to voice (Gr. *φέρει*, Hitt. *-ta* and Gr. *-σθα* in the active voice: the perfect in *-a* being indifferent); the fact that there are 'tantum' forms which are only either active or middle voice; that the opposition did not spread to languages such as Baltic and Slavic (although there is a theory, which I believe is erroneous, that it was lost) (Autor 1981: 41 ff.).

Thus, binary lexical oppositions were the nucleus for binary oppositions of series of roots provided with grammatical elements (endings or ∅ endings) either concommitant with mentioned oppositions or used secondarily to express those. There comes a moment when only the opposition of endings added to one and the same lexical item is sufficient.

There is a certain difference in the opposition of number (singular/plural). Here, too, there are 'tantum' lexical forms (either only singular or only plural); but they are not opposed to each other and there are, on the other hand, syncretic forms which are not yet differentiated: this is what often occurs in Hittite inflexion. In number the opposition of endings, when it is there, comes from secondary differentiations: this is how I have interpreted the oppositions *-os/-ōs* (with metrical lengthening), *-os/oi* (with agglutination of *-i*), *-s/-es* (with difference of vocalic degree), etc.; and *-m/-ms, bhi/-bhis* (with secondary interpretation of *-s* as a plural marker). Probably, originally anumerical nouns were characterized as singular or plural when they were determined by lexical items of these types ('one'/'two', 'three . . .', 'I'/'we'. . .) (cf. Adrados 1975: 427 ff; 1984 b)

In the last instance, these are re-makes of lexical oppositions with the aid of endings. But in others cases, an ending has also been used with its own value as against another ∅ ending which has become the negative term: thus in the temporal opposition (*-mi/-m, -si/-s* . . .), it is necessary to point out that the negative term may act as a positive one (the type of Gr. *τίθης*, Hitt. *arta* as present forms) and that the *-i* may be extended outside its limits. The use of an ending to mark one term of an opposition as against the lack of ending to mark the other is also found in the indicative/imperative opposition as it appears in Hittite (although the negative term acquires certain formal markers secondarily).

To sum up: the basis of the binary oppositions is in lexical or grammatical pairs (specific ending as against ∅) or in the abstraction that opposes "one" to the other numerals. Or in the combination of different resources: at times, by attributing to both opposed terms forms which were in principle indifferent to opposition.

3. What happens with the multiple oppositions of Anatolian can now be understood.

The first is that of person: first, second and third. Obviously, this is based on the existence of the pronominal forms 'I', 'you' and the other pronouns and nouns in general. On the other hand, it has no lexical basis: all verbs have the three forms.

However, the partial identity of the second and third persons singular is only too well-known. In fact, in Greek, within the forms which were originally without endings, *φέρεις*/*φέρει* were only secondarily differentiated; in Lithuanian something similar occurred, but here the differentiation was carried out with the aid of vowel timbre (2nd *-e* < **-ei*/3rd *-a* < **-o*). When there are endings, things are even clearer: in Hittite in the preterite we find *-t*, *-š*, *-ta* and *-šta* in both persons and there are parallels in other languages (cf. Adrados 1974: 621 ff., 635 ff.). In fact, second-third forms were secondarily split, certain ones being used for the second, and others for the third; they were old allomorphs.

That is to say that the ternary opposition was carried out by means of a scission within one of the two terms, the negative. Once more, we come to the same result. And if we consider that the opposition existing between base verb/iterative/causative was a ternary opposition, then the evolution which led to this result is here different: base verb/iterative and base verb/causative are the two underlying binary oppositions which were then combined. In each one of them a form considered to be non-characterized was opposed to another characterized one following a pattern that is by now well-known to us. In this case it is clear that an opposition between stems was used for the first time, which has been considered as a model for all later ones (cf. Rosenkranz 1979: 227).

I believe that this combination in a multiple opposition of older binary oppositions is what happened as far as the case system is concerned. Naturally, I am not starting with the theory of an older system of eight cases which were later syncretized here and there; in this sense I would refer to my above-mentioned works and to Villar (1974), as likewise to Lehmann (1958: 179–202), Kuryłowicz (1964: 190 ff), Fairbanks (1977: 101–131), Schmalstieg (1980: 73 ff). Today, there is a widespread opinion that the system of Nominative, Accusative, Genitive and Vocative is older, as likewise a form variously interpreted either as a Locative or as an extra-syntactic one; for Villar and myself, the Dative/Locative split is secondary.

However, without going further into the creation of the system, as it basically already existed in Indo-European 2, one should note that if we believe that it is the synthesis or combination of various systems of binary oppositions, this is because each of them belongs either to a different function of the language or to different contexts. I believe that there were initially several oppositions:

a) A functional opposition: base noun/Vocative
It is clear that the vocative inherits a special use of the noun in expressive and impressive non-representative function: it was formalized by means of junctures, initial accent placing, the use of the pure stem, etc.

b) An Adverbal opposition (created within the base noun)
The Nominative/Accusative opposition refers to that existing between two actants or determiners of the verb: this is its context. Of course in the Nominative there were still noncasual uses (asyntactic Nominative *pendens*, etc.) and the Accusative at times acquired new contexts secondarily (Accusative of relationship to the noun, adverbial Accusative). The opposition is expressed by means of endings (including the ∅ ending), which in general mark these two types of verb determination (Adrados 1975: 407 ff).

c) An adnominal opposition
The base noun/Genitive opposition opposes any noun to another one which has the function of noun determiner. The Genitive as verb determiner is secondary as is generally believed.

d) A sentence opposition
The remains of the base noun which were not split into the three former binary systems were used with a local value opposed to any other noun in the sentence. I shall not enter here into a discussion of the traditional theory that there were originally four cases (Dative, Locative, Instrumentalis and Ablative); I believe that the Dative/Locative scission is secondary (their markers -∅, *-i* and *-ei* are the same) and that the Ablative and Instrumentalis were developed as dialectal. I refer to the above-mentioned publications to this end and for all details concerning the formalization of the different oppositions.

It is clear that the evolution of Indoeuropean tended to confuse functions and distributions and to combine the four binary oppositions in one single system. Opposition c), for example, as from a certain moment onwards, opposed to the Genitive both the Nominative, the Accusative, and even the Vocative and Locative; all cases come into the negative term opposed to the Vocative, and, of course, into that opposed to the Locative.

Although very briefly expounded, I believe that only thus may the origin of the multilateral case system be explained. Despite all, it has preserved traces of the following archaisms: the limitation of certain cases to certain distributions (or preference for them), the opposition of certain cases to others (and not to all of them) also in certain distributions, systems of partial oppositions within the overall case system.

Thus, Indo-European 2 not only provides proof that the oldest systems are the binary ones, and specifically those created with either lexical or ending elements, but that it was the first which resources that the whole of Indo-European was thenceforth to handle in order to create systems of multiple oppositions: either by splitting one term of an opposition or by synthesizing several into one, and by using the opposition of stems as a morphological resource as well as the former ones.

4. This brief account of the creation of systems of grammatical oppositions in Indo-European 2 may serve as a guide for tracing another parallel with regard to Indo-European 3: I have already stated that there is an extension of the morphological resources (generalized use of stem oppositions) and that at times this is an extension of a binary system in Indo-European 2 which as a consequence becomes ternary or multiple, at others creates a new system – here, too, as always, on the basis of a binary one.

There are a series of concepts which I shall handle – concepts which partly correspond to already known data in Indo-European 2 – and which should be more precisely defined. I speak of scission when one term of a binary opposition is split into two terms that are opposed to each other: the allomorphic stems of which it consisted are shared by the two

terms of the secondary opposition now created. Binary opposition thus becomes ternary. This is what already occurred in Indo-European 2 when the person opposed to the first was split into second and third as we have seen.

The concept of allomorphization means that different stems become part of one and the same term of an opposition that is created. Thus, this occurred already in Indo-European 2 in the case of the animates and the inanimates, for example.

I speak of combination when two or more binary oppositions with a common term are combined into a multiple one: just as in the case system which already existed in Indo-European 2.

Finally, the concept of addition means that, at times, one or more terms of an opposition are secondarily opposed to a new term, whatever its origin may be. The opposition thus becomes ternary or multiple. This is somewhat parallel to what occurs when a term is opposed to another to make up a binary opposition; it is therefore a continuation of the same procedure.

Let us first briefly examine the extensions of the old binary oppositions of Indo-European 2 mentioned above. For more detailed analysis, I refer to the above-mentioned bibliography and to the already quoted works. We find:

a) Addition

This is produced with ending elements when the plural in a series of Indoeuropean languages becomes a negative term opposed to a newly created positive one, the dual, which refers to certain numerical uses (two individuals or objects in certain circumstances), which are also expressable as a negative term by the plural. With regard to the origin I suggest analogy with **duō* and **ambhō* in the case of *-ō(u)* for the dual endings (cf. Adrados 1975: 440 ff).

b) Scission

Animate nouns split into masculine and feminine: certain lexical items and often stems followed by certain grammatical elements (above all masc. *-o* / fem. *-ā*) form two opposed series by means of a process of allomorphization. As is known, the feminine is a positive term and the masculine a negative one. On the marking of masculine and feminine (respectively) with elements such as *-o* and *-ā*, which at first were indifferent to this distinction, several hypotheses may be formulated (cf. Adrados 1975: 481 ff).

c) Scission and addition

Both resources are used supplementing each other in the extension of the mood and voice systems.

The stems which in Anatolian (as the representative of Indo-European 2) function as allomorphs of the indicative, that is, of a non-imperative, are classified in Indo-European 3 as indicative stems and subjunctive stems: in both terms a process of allomorphization takes place. I have explained elsewhere[6] that there are no stems which are specifically of the indicative or the subjunctive kind: The definition is proportional and this can be seen very clearly when one touches upon the subject. By the time, however, the well-known fixed correlatives of athematic indicative/thematic subjunctive or that of thematic indicative/subjunctive with long vowel were established. Authors like Renou and Kuryłowicz by now

acknowledge at least the identity of the *-e/o* of the indicative and that of the subjunctive (cf. Kuryłowicz 1964: 137 ff; 1977: 90 ff.).

Thus a ternary system was formed. A positive term corresponding to the impressive function of the language (the imperative) had a negative one in which this function coexisted with the representative one; then the above-mentioned split created a secondary opposition of indicative/subjunctive. However, on the other hand, there is addition of the optative, which has a characteristic suffix *-iē/-ī* (also in the *-oi-* of the thematic verbs). This four-term system is highly complex: the indicative indicates reality as against the subjunctive-optative, and there is a gradation of reality (indicative)/eventuality (subjunctive)/possibility (optative); at the same time, there is an opposition between mandate (imperative)/will (subjunctive)/wish (optative) within the impressive values which these moods preserved, as compared to the indicative.

One would achieve similar results by analysing the creation of a passive voice which makes the binary voice system (active/middle) a ternary one; although this is not an Indo-European phenomenon since the different languages develop this independently, it is clear that the old middle voice at times can be split into a middle and a passive one, whilst at others the passive received the addition of special elements.

d) Combination

The evolution of the tense system in Indo-European 3 consists of two phases which are chronologically different.

In the older of the two, the present/preterite opposition, which belonged to Proto-Indo-European (Indo-European 1), takes a new formal characterization which does not cancel out the older one (based on endings): certain stems are assigned to the present, others to the preterite. The distinction is only proportional to a great extent and often dialectal; however, as in other parallel cases, certain stems tend to be assigned to one of the opposed terms, others to the other (cf. Adrados 1975: 675 ff.). The whole of this formal re-shaping hardly affects the semantics of the opposition, except that one must assume that is was formerly a privative one (*-m, -s* . . . as the negative term) and that it is now an equipollent one, for both the present and certain preterites in certain Indoeuropean languages display atemporal uses.

The second phase is that in which, at least in Greek, Indo-Iranian and Baltic, the present/preterite tense opposition was combined with the atemporal present (negative term)/desiderative opposition, thus a positive future term being created. As is known, the synthesis came about through the proximity of the semantic values of the desiderative and the future and through the existence of a 'case vide' for the future.

Once more, we observe in all cases the secondary nature of the ternary and multiple oppositions in general and their construction through lexical elements, and above all stems. There is only the exception of the dual, which was created on the basis of an opposition of endings.

5. The new oppositions are still to be discussed, as I mentioned above: as I stated then, they were all created on the basis of opposing stems. There are processes of allomorphization of existing stems (and other new ones which were created) which contract binary oppositions that later give ternary ones.

a) Scission (split)

I believe this is what occurs in verbal aspect. Leaving aside that based on the opposition of base verb/verb with preverb as likewise the diverse 'Aktionsarten', whose existence in Indo-European 2 I already mentioned, I shall keep to the aspect which comes into play between the present, perfect and aorist stems. The present/perfect opposition starts from that between diverse allomorphic present stems and a deverbative of state.[7] In fact, the result is an opposition between a base verb and a special stem with stative value.

But this base verb (the ensemble of allomorphized stems) splits into present and aorist by means of a new process of allomorphization in both sectors. The starting-point is plausibly in certain iterative and durative stems and in other punctual ones, which become crystallization points around which the two opposed terms are made up. Thus, the oldest and most general binary opposition is extended to make up a ternary one (cf. Adrados 1974:675 ff.).

b) Combination

The degrees of comparison in the adjective (positive, comparative and superlative) make up a ternary system resulting from the combination of two previous binary systems, the positive/comparative and the positive/superlative.

It is well-known that these are sometimes lexical oppositions, sometimes grammatical ones (with diverse allomorphized suffixes), and sometimes mixed ones. It is clear that the first step is allomorphization (as is known, for example, *-tero* and *-ios* originally had quite different values) and the constitution of the two binary systems with a common term (the positive). Their combination makes up a ternary system of the gradual type, as to a certain extent that of the Indicative/Subjunctive/Optative (reality/eventuality/possibility) and that of Imperative/Subjunctive/Optative (mandate/will/wish), although clearer.

As was stated, these new systems of Indo-European 3 display defective data, among others, which definitely prove their secondary nature.

6. I think the foregoing is sufficient, although similar arguments may be put forward for the 'Aktionsarten', deixis of the pronoun, etc., always along the same lines.

If Proto-Indo-European or Indo-European 1 was a non-inflexional language, as is thought, an intellectual mechanism must be thought up to explain how later categories were created: obviously with the aid of formal elements prior to these and originally indifferent to them. The process took place in stages: the creation of the binary oppositions, the extension of these latter, the later creation of other binary and ternary ones and their extension. Fundamentally, the procedures were always the same: if we follow up the process in the individual languages, we shall see how it is repeated. There is, however, an evolution: first the lexical and ending resources are used, whereas the stems are hardly used; then, without renouncing the former, the latter become the more frequent.

In all cases, abundant irregularities are left which may serve as a guide in the reconstruction of the oldest oppositions and the development of the system through its various stages. I have given a more detailed account of these irregularities and their use to this reconstruction elsewhere (Adrados 1984b).

Notes

[1] Cf. among others: Adrados 1974, Adrados 1975, Adrados 1981a: 96–122, Adrados 1982: 1–35, Adrados 1985a und 1985b.

[2] Above all in Adrados 1985b.

[3] Cf. Kuryłowicz 1956, and above all Kuryłowicz 1964 and Kuryłowicz 1977. Cf. a criticism indicating the points which differentiate my work from that of this author in Kuryłowicz 1974: 932 ff. and Adrados 1982: 195.

[4] Adrados 1962: 5–41, Adrados 1966: 131–154, Adrados 1968: 1–47.

[5] That Anatolian had no masculine/feminine opposition is becoming acknowledged by all. Cf. lately Laroche 1970: 5–57.

[6] Adrados 1974: 849 ff.; Adrados 1975: 7/9 ff., in which the supposed secondary loss of the subjunctive in Baltic and Slavic is also refuted.

[7] For the origins of the perfect and its non-existence in Anatolian, cf. among others Nadia van Brock 1964: 119–165 and Adrados 1981b: 27–58.

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Syllabic prominence in ancient Greek: a typological approach

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It is almost universally accepted (disbelief being practically confined to modern Greek speakers) that the accent of ancient Greek, up to about the third century a.D., was of melodic type, manifested by a rise of pitch at a particular place in the word. The evidence for this has been widely discussed: it includes the testimony of ancient writers, philosphical, critical, and musical, supported by the implications of their terminology; of ancient Greek musical settings, notably in certain of the Delphic hymns; and of parallelism with the related Vedic accentuation, which was similarly described by Old Indian phoneticians. There is also support of a typological nature if one accepts Trubetzkoy's generalization (1939: 213) that those languages whose prosodic phonology is most simply described in terms of the 'mora' characteristically employ pitch as the accentual feature; for the rules governing the location and form of the accent in ancient Greek are certainly most simply stated by treating long vowels and diphthongs as comprising two units (morae), as against the one of short vowels (cf. Jakobson 1962: 263; Allen 1973: 234 ff.).

It is further indicated that the melodic peak did not involve any appreciable increase of intensity or duration such as may characterize a dynamic (stress) accent. For no account is taken of the position of the accent in the composition of classical Greek verse; whereas when later the accent changes to dynamic type, it does play an increasing and ultimately exclusive rôle. One result of this is that readings of ancient Greek verse by modern Greek speakers, which commonly stress the accented syllables and make no distinctions of phonemic vowel-length, thereby destroy any element of regular rhythm: a line such as *Iliad* i.84, for example, of which the quantitative pattern is

–oo–oo–oo–oo–oo–– (where –/o = heavy/light syllable),

being read as

[tón đ'apamivó·menos prosé·fi pó·đas ocí·s açiléfs] (where ' now indicates stress, and · automatic vowel-lengthening in stressed open syllables),

in other words with a 'rhythm'

'xxx'xxx'x'xx'xx'
followed in the next two lines by x'x'xx'xx'xx'x'x and x''x'xxx''x'x''x.

Not quite the same agreement prevails regarding the typology of the classical Latin accent, but the majority opinion is certainly that it was of dynamic type, in spite of the statements of Latin grammarians based on Greek tradition. Some of those who appreciate the evidence for a dynamic accent but are unwilling to dismiss the grammarians' evidence entirely have had recourse to a compromise solution. They note that many descriptions of 'stress' in

modern languages suggest that it is not simply, or even principally, a matter of acoustic amplitude or auditory loudness, but is commonly also associated with variation of duration and particularly of pitch. Therefore, it is argued, we should not make a sharp distinction between the two types of accentuation, since changes of pitch are common to both (cf. Schmiel 1981: 22, with further references). This argument, however, obscures an important issue. It is one thing for pitch to be the primary feature of an accent, and quite another for it to be an incidental cue to some other physiological activity on the part of the speaker, namely what we conventionally call 'stress'. Precisely what is involved in the latter is much disputed: one hypothesis that has found favour is that of Öhman (1967), who suggests that stress involves "the addition of a quantum of physiological energy to the speech production system as a whole... distributed (possibly unevenly) over the pulmonary, phonatory, and articulatory channels". The marked pitch component in stress is readily explained by the fact that "the laryngeal musculature is often affected by tensions in the other musculatures of speech" (Stetson 1951: 95; for more specific details cf. Lehiste 1970: 82, 125, 144).

As regards the distinction between the Latin and Greek accents, what the compromise solution overlooks is the lack of any evidence for cues of duration or amplitude in the ancient Greek accent – just as in modern pitch-accented and tonal languages fundamental frequency alone generally provides "the necessary and sufficient perceptual cue" (Gandour 1978). It also overlooks an important structural difference as shown by the rules governing the location of the accent in the two languages. In Latin the position of the accent is governed by the so-called 'penultimate rule': if the penultimate syllable is heavy it is stressed; if it is not (in words of three or more syllables) the antepenultimate is stressed, whether heavy or light – thus *re-fḗ-cit, re-féc-tus*, but *dḗ-fī-cit, ré-fī-cit*. It is to be noted that it makes no difference whether the heavy quantity of the penultimate syllable is 'by nature', i.e. on account of its containing a long vowel or diphthong, or 'by position', i.e. on account of its consonantal closure. In Greek, where the accent is relatively free, there are certain limiting rules; but here the determining factor is not generally the quantity of the syllable but the length of the vowel. A further difference from Latin is that in Greek a long vowel or a diphthong may have two forms of accent, the acute as in *oíkoi* 'at home' or the circumflex as in *oîkoi* 'houses', the high pitch in the former being located on the second mora and in the latter on the first (equivalent to *óikoi*). No such distinction prevails in Latin (*pace* Pompeius and Priscian), where the concept of the 'mora' is irrelevant (*pace* Trubetzkoy and Jakobson: cf. Allen 1973: 161 f.).

Regardless, therefore, of the actual phonetic characteristics of the Latin accent in particular, it is clear that the accents of Latin and of Greek were quite different in terms of structural typology; and provided that we do not insist on the precise physiological implications of their titles, we may continue to distinguish them in the traditional terms of 'dynamic' versus 'melodic'.

It is however also the case that no languages, even those with melodic accents (or with distinctive tonal systems), lack patterns of non-melodic 'prominence' (to use a non-committal term) – just as no languages, even those with dynamic accents, lack patterns of pitch variation, although in both cases these secondary patterns may not necessarily be linked to the accent: I have elsewhere (1973, 94 f.) listed a wide range of spoken languages which illustrate this principle. Patterns of this kind are not marked in orthographies, since they do not have the same differential functions as, say, the vowels and consonants, and so are

not directly traceable in the case of dead languages; and for the same reason contemporary writers may make no reference to them even though they discuss accentual patterns which may equally be unmarked. In the case of Latin we know nothing about non-accentual patterns of pitch-variation, such as may have characterized the intonation of sentences; and in the absence of any direct testimony it is difficult to see how we could set about recovering them. But it is at least possible in principle that some indications could be recovered of non-accentual patterns of prominence in Greek. For, as Latin poetry suggests, there may be a tendency, even in quantitative verse, to seek coincidence of prominence (accentual in the case of Latin) with the strong position of the foot, i.e. with that element of the foot which is normally marked by a heavy syllable. If this were the case in Greek, then the placement of words in the verse should give some indication of any (non-accentual) prominence-patterning amongst their syllables.

Many words would of course be irrelevant to such an enquiry, since their quantitative patterns would automatically restrict them to particular places in relation to strong position, so that their placement would not necessarily indicate any element of prominence; this applies to all words in which a heavy syllable is flanked (on either or both sides) by one or two light syllables. But other types of word, containing only heavy syllables (or a succession of more than two light syllables in the case of 'resolved' feet) could provide relevant evidence, since their quantitative patterns permit alternative placements in relation to strong position. And even in the case of the former type, the 'tied' word-patterns, it would be suggestive if any of them were avoided in particular positions, or only admitted 'faute de mieux'.

I have argued elsewhere (1973: 283 ff.) that an investigation of this type in spoken Greek verse reveals certain very clear strategies of placement, which may be summarized (as I have not previously done) in the following diagrammatic formula, showing how various types of word (or word + clitic combinations) tend to be placed in relation to the strong positions of the line (S), including those which for quantitative reasons must necessarily be so placed if at all; optional syllables are set in parentheses:

$$\underset{\text{oo}}{\underline{\text{S}}}\,(\text{o})\;\_\;\underset{\text{o oo}}{\underline{\text{S}}}\,(\text{o})_{\text{o}}$$

In deriving particular word-patterns from this formula by working from right (end of word) to left, one may move (but not necessarily) from one row to the other, with the proviso that restrictions on sequences of light syllables in verse preclude movement from (o) to o. There is no spoken metre which could provide examples of word-patterns $(\underline{\text{S}})_{\text{o}}\;\_\;\underline{\text{S}}^{1}$ although they are derivable from the formula; and notable patterns *not* derivable from the formula are:

$$\underset{\text{oo}}{\underline{\text{S}}}\;/\;\underline{\text{S}}\;\_\;/\;\underset{\text{o oo}}{\underline{\text{S}}}\;/\;\_\;\underset{\text{oo}}{\underline{\text{S}}}\;/\;\underset{\text{oo}}{\underline{\text{S}}}\;\_\;/\;\underset{\text{o}}{\underline{\text{S}}}\;\_\;/\;\_\;\underline{\text{S}}\;\_\;/\;\underset{\text{oo oo}}{\underline{\text{S}}}\;/\;\underset{\text{oo oo}}{\underline{\text{S}}}\;/\;\underline{\text{S}}\;\underset{\text{o oo}}{\underline{\text{S}}}\;/\;\underset{\text{o oo}}{\underline{\text{S}}}\;\_\;/\;\underset{\text{oo}}{\underline{\text{S}}}\;\_$$

These non-derivabilities reflect, *inter alia*, the constraints of 'Porson's Law'[2] in iambics and trochaics and of 'Gerhard's Law'[3] and 'Naeke's Law'[4] in hexameters.

In interpreting the formula it is to be noted that it assumes the application of the traditional 'law of indifference', whereby at the end of a line of verse a heavy syllable in terminal weak position is treated as light, and a light syllable in terminal strong position is treated as

heavy. Thus a hexameter may end with a word(-end) $\_\ \_$ (placed $\underline{S}\ \_$) because it is equivalent to $\_\ o$ (= catalectic dactyl), and an iambic trimeter or catalectic trochaic tetrameter may end with a word(-end) oo (placed $o\underline{S}$) because it is equivalent to $o\_$. When this is taken into account, the derivations from the formula agree with the data in 90–95% of cases, and virtually 100% in the coda of any line.

The exceptions concern primarily placements of the type $(\ldots)\ \frac{S}{oo}\ \_$. The most notable of these is the position preceding the penthemimeral caesura of iambics, where there may be special considerations. There are also the exceptions to 'Naeke's Law' in Homer (as against Callimachus), involving principally the placement of words or complexes of pattern $(o)o\_\_$ ending with the 4th foot, where however there is clear evidence of a 'faute de mieux' principle at work (cf. Allen 1973: 286 ff.). Other rather frequent exceptions concern the beginnings of lines (e.g. placements $o\ \underline{S}$ in iambics,, $\underline{S}\ \_$ in hexameters); but rhythmical irregularities and inversions are here a general commonplace. Another less common exception is the placement $\underline{S}\ o\ \underline{S}$ in iambics, but here there are special constraints on the nature of the final two syllables (cf. Allen 1973: 323 f.).

The fact remains that there is a very clear preference for particular placements of words relatively to the strong positions of the verse line, as summarized in the above formula; and it seems most likely that these preferences reflect certain patterns of prominence in Greek words (unrelated to the accent), and more specifically that those elements which are preferably placed in strong position were in some sense prominent in the language. From the placement formula we could then derive the following rules for the incidence of prominence in Greek words:

1. Prominence applies to an element constituted by either (*a*) one heavy syllable or (*b*) two light syllables.
2. Words (or word-like sequences) longer than an element have internal contrasts of prominence/non-prominence.
3. If the final syllable of a word is heavy, it is prominent.
4. If the final syllable is light, the next preceding element is prominent.
5. A preceding element separated[5] from the prominent element is also (secondarily) prominent.

In earlier discussions I have suggested that 'prominence' is here to be equated with stress – though presumably weaker than the accentual stress of a language like Latin (as is also suggested by its effacement by the accentual stress which later replaced the melodic accent). Some scholars have been convinced by these arguments; others, however, whilst accepting that the preferences of placement indicate some form of prominence in particular syllables of words, would contest the conclusion that this was stress: enhanced duration or raised pitch have both been proposed, but not as cues to stress. I have briefly mentioned (1973: 292) some objections to these counterproposals; but there must inevitably remain an element of subjective judgement in assessing probabilities unless more positive clues can be uncovered.

We might consider the patterns of assumed prominence expressed in the Greek formula as a kind of typological 'fingerprint' of that type of prominence. And if it were possible to find a matching fingerprint in a language where the phonetic nature of the prominence was established, we should have quite a strong typological argument for concluding that the prominence in Greek was of the same nature. And it is at this point that we return to the question of the Latin accentual system.

The traditional 'penultimate rule' of Latin, whilst descriptively adequate, has certain weaknesses in respect of explanatory adequacy (in the sense of Chomsky 1965: 27). For it implies that, whereas an antepenultimate light syllable is accentable under the same conditions as a heavy, viz. if followed by a light syllable, a penultimate light syllable may not be accented (except in disyllabic words). It might be and often has been suggested that the accentuation of an antepenultimate light syllable is simply a 'pis aller' in the absence of any heavy syllable in the accentable portion of the word. But words of this accentual pattern are very common, being no more constrained than any other, and it is explanatorily unsatisfactory to treat them simply as exceptions.

Quite independently of the Greek question I therefore attempted some time ago (1969) to formulate a more satisfactory rule from this point of view. It involved the concept of an accentual 'matrix', which comprised either one heavy or two light syllables; phonetically the peak of stress in a disyllabic matrix would be reached on the first syllable, just as it is reached in the first part of a heavy syllable, following the commonly 'diminuendo' pattern of stress. We could therefore represent the accented matrices as $\hat{\text{o}}$ or ó ò respectively. In the latter the first of the two syllables (carrying the stress-peak) would correspond to the syllable traditionally described as accented. On this basis the location of the Latin accent (in words of three or more syllables) may be stated as follows:

The accent occupies the last matrix in the word preceding the final syllable.

The last part of this rule excludes accentual patterns such as $\bar{\text{o}}\ \bar{\text{o}}\ \hat{\text{o}}$ or $\bar{\text{o}}$ ó ò. As stated in these terms there is nothing peculiar or exceptional in the accentuation of e.g. *ré-fi-cit*, which is now analysed as ó ò $\bar{\text{o}}$, accented on the matrix o o, just as e.g. *re-fḗ-cit* is analysable as o $\hat{\text{o}}$.

The rule for the location of the Latin accent, when thus stated, may be represented as follows (where A = accent):

$$(\ldots)\ \underset{\text{oo}}{\text{A}}\ (\text{o})\ \bar{\text{o}}$$

The rules of derivation from this formula are the same as stated for the Greek S-formula above, producing word-end accentuations

$\underline{\text{A}}$ o $\bar{\text{o}}$, o^{A}o $\bar{\text{o}}$, $\underline{\text{A}}$ $\bar{\text{o}}$ (in traditional terms $\acute{}$ o $\bar{\text{o}}$, ó o $\bar{\text{o}}$, $\acute{}$ $\bar{\text{o}}$).

The word-end pattern $\underline{\text{A}}$ $\bar{\text{o}}$ will also apply to disyllabic words of this shape (thus e.g. *rā́mus* as *amā́mus*).

It will be seen that this formula bears a remarkable similarity to the end of the Greek S-formula. The only difference is accounted for by the irrelevance of final quantity in the Latin accentual system. Other languages having a similar system to that of Latin are Sanskrit (after the loss of the melodic accent), the modern Indo-Aryan languages, and classical Arabic; and in the last two of these final accentuation does in fact occur in the case of overweight syllables (-V̄C or -V̆CC). Latin does have a few cases of final accentuation of such syllables as the historical consequence of contraction, syncope, or apocope (e.g. *fūmā́t* < *fūmā́uit; nostrā́s* < *nostrā́tis; illī́c, illī́nc* < *illī́ce, illī́nce*), but there is no synchronic rule to this effect[6] (thus e.g. *laúdās, laúdant*).

It is generally assumed that in longer Latin words there were also secondary accentual patterns in the pre-(primary-)accentual portion (see e.g. Fraenkel 1928: 351 f.). If we assume that the accentual rules represent in some sense a limitation set on the span of

prosodically undifferentiated sequences, an a priori hypothesis would be that the pre-primary-accentual part of a word was treated as a word for secondary accentual purposes. And in fact for classical Arabic the following rule is stated by Abdo (1969: 73a):

"Starting from the segment immediately preceding the stressed vowel, apply the rule once more to any segments that again meet its structural description. The vowel stressed in the second application receives secondary stress".

Thus e.g. *kátabáthu, ziyáratuhúnna*, and with tertiary stress *wáyatakátabúna.* Metrical evidence at least does not contradict this assumption for Latin (e.g. in iambics/trochaics *súspicábar, indíligéntia, uínōsíssima, mísericórdia*; and at hexameter-end *Láodamía, ábscondántur*); and if we extend the Latin formula to include secondary accentuation, the similarity to the Greek formula becomes the more striking: thus:

$$\underset{\bar{o}\bar{o}}{A}\,(o)\,\bar{o}\,\underset{\acute{o}\bar{o}}{A}\,(o)\,\bar{o} \quad \text{beside} \quad \underset{\bar{o}\bar{o}}{S}\,(o)\,\bar{o}\,\underset{\acute{o}\bar{o}}{S}\,(o)_{o}$$

With the one exception noted, the formulae are identical.

The Greek placement rules gave rise to the hypothesis of some factor of prominence in those elements of words placed in the S position. Typologically therefore it is at least a reasonable conclusion that the elements of Greek correspond to the matrices of Latin, and that S corresponds to A. The factor of prominence involved in A is generally agreed to be stress, and it would be a rather remarkable coincidence if the prominence factor associated with S were of a totally different kind. The exact correspondence of the disyllabic stress matrices and disyllabic strong positions may then also suggest a phonetic explanation of the phenomenon of resolution.

By contrast, the limiting formula for the recession of the melodic accent of Greek would bear little typological resemblance to the A/S formulae. As we have seen, it is based on vowels rather than syllables; it involves inter alia the concept of the 'mora', and (for its most economical statement: cf. Allen 1973: 234 ff.) of a 'contonation' comprising either the two morae of a long vowel or a diphthong (= the marked circumflex) or else the second or only mora of the vowel plus the next vowel in the word (= the marked acute): the rule is then statable that 'not more than one mora may follow the contonation': thus e.g.[7] *lupóù, dóòron, mónòn, hóloòs, baínoò, ánèmos, ánthroòpos* are admissible, but not e.g. **dóòrois* or **ánthroòpois* (correct are *doóroìs, anthroópoìs*).

The one disagreement between the Greek (S) and Latin (A) formulae also deserves some further notice. In the Latin formula the quantity of the final syllable is indifferent, but not so in Greek; consequently in Latin the final syllable is non-A whether heavy or light, but in Greek it is non-S only if it is light. This means that in Greek, unlike Latin, there is the possibility of alternative placements of certain words, viz. those ending in -V̆C, depending on the initial of the following word: thus e.g. the word *nēas* in

Il. i.306 $\underset{\bar{o}\bar{o}}{S}$ *nēas* $\underset{\bar{o}\bar{o}}{S}$ *ēīsās*

but *Il.* ii.493 $\underset{\bar{o}\bar{o}}{S}$ *neās te propāsās* $\underset{\bar{o}\bar{o}}{S}$

which suggests the possibility of shifting prominence according to environment. This is something which does not occur in languages like Arabic and Indo-Aryan, which permit final accentuation, since only 'overweight' syllables are thus accented, and these by their nature are not subject to syntagmatic variation of quantity (the same applies to the few

cases of final accentuation in Latin). But the crucial point here is that in Latin and these other languages we are dealing with fixity of accentual placement[8], whereas the assumed Greek prominence patterns are non-accentual, and it would not be surprising if syntagmatic variation in these were found: rather as in a dynamically accented language like English words may bear different melodic (intonational) patterns according to their environment. In other words, in Latin and similarly accented languages we are dealing with specifically word-patterns of stress (just as with the pitches of the Greek melodic accent), whereas in Greek we are dealing with sentence-patterns of prominence, albeit regulated by word-boundaries.

Conclusion:

The rules governing the melodic accentuation of ancient Greek are quite different from those governing the dynamic accentuation of Latin. But the rules relating the strong positions in spoken Greek verse to the quantitative patterns of Greek words are almost identical with those relating the Latin accent to the quantitative patterns of Latin words. This detailed coincidence in independent prosodic systems suggests that the physiological factor underlying both sets of rules is the same. If in the case of Latin this was stress, it is reasonable to think that the same factor underlies the Greek rules, though in this case it has no accentual function.

Notes

1 The pattern o $-\underline{\underline{S}}$ in iambics and trochaics is precluded by rules constraining word-division in resolution.

2 R.P., *Hecuba*^{2} 1802, Suppl. ad praef., xxx.

3 E.G., *Lectiones Apollonianae*, 1816: 142 f.

4 A.F.N., *RhM* 3, 1835: 516 f.

5 I.e. by a heavy or by one or two light syllables, of which the first and last, though themselves elements, are not separated from the (primarily) prominent element.

6 For further discussion cf. Allen 1983.

7 To conform with the moric basis of the rules long vowels are indicated by double writing. The coda of the disyllabic intonation may be assumed to occupy the whole of the vowel/diphthong involved, i.e. to return to the non-high pitch level towards the end of it (as in the case of the monosyllabic contonation); the grave sign is therefore marked on the second element of the long vowel or diphthong.

8 It might even be argued (cf. Allen 1983) that the limitation of final stress to overweight syllables has the specific function of ensuring such fixity.

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Hittite *pāi-*, why no **wāi-*: an Anatolian-Indo-European heterogloss[1]

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This little article honors my one-time professor whom I neglected as a student, giving his Greek Dialects class but the preparation time that Akkadian and Sanskrit left (which meant c. 30 minutes from an 18 hour study day).

Those of us who consider Anatolian as equivalent in worth to all of Indo-European Proper (IEP) together for the recovery of the earliest parent language have received our share of sneers. The view, which waxes and wanes in its intensity, that Anatolian 'discrepancies' vis-à-vis IEP are to be ascribed to Anatolian's being a pidginized IE (sic!), from whose information bits we can explain away embarrassments for Brugmannian IE[2] by declaring the item in question to be sub-/ad-/superstratic material, dominates amongst those who hold the major seats on 'The Board of IE Trustees'. In the allotted space I cannot approach this grand question, which – at all events – is more one of authority than of scientific quest. It has even proven impossible to deal with the three Anatolian lexical bits, whose cohesion formed the foundation into which the single bit to be discussed herein, fits. Given these realities, I will devote this space to my third utterance[3] on the Anatolian verb 'to give' (*pāi-/piya-*).

I deem it worthy of first place to note that, in either camp described above, one ineluctable actuality pertains for any Anatolian lexical bit: a choice which so far has been conceptualized as only tripartite:

(a) a word or morpheme is obviously common to Anatolian and IEP;
(b) the Anatolian word is obviously borrowed from Hattic, Hurrian, Semitic, or unattested substrat languages (Kultur- und Wanderwort often);
(c) the Anatolian word has neither genetic cognates nor a donor source[4];

The model, that the evidence accumulated for this paper (but not spatially "presentable") has led to, holds that there is a fourth alternative, one which sadly has been underutilized;

(d) Anatolian may present the lexical bit in an appearence sufficiently different from its IEP cousins, that the family resemblance goes unrecognized[5]. When this choice is taken, the equation can only stand when its vitality (1) **is not vitiated** by internal Anatolian patterning and (2) **is supported** by parallel Anatolian: IEP heteroglosses.

1. History of the Proposals

In my article (1976: 79–80) I offered a triad of reasons that renders the now canonical analysis of *pāi-/piya-* into the constituent elements, preverb *pe-*[6] + √*ai-*, untenable. One – not the least – of my three causes for dubiety was the absence of (a) a directionally contrasting verb **wāi-/wiya-* 'receive' and/or (b) a simplex **ai-/iya-* 'exchange', in contrast to *pāi-* 'go', where (1) Hittite has (a) the contrast *pāizzi/wizzi (pe-/u-* + √*ei-)*, (b) remnants of the simplex (e.g. the imp. *i-t*); (2) Cuneiform Luwian has an *i-/awi-* contrast (see the references in my article [1974: 71–72]); (3) Hieroglyphic Luwian has the simplex (e.g. the [rhotacized] 3rd sg. form *i-ri* 'goes') and the form prefixed by the frozen adverb *aw-* (3sg. *aw-i-ti* 'comes'). As I saw the situation, one of the signs for securing a definitive etymological analysis of an Anatolian verb as belonging to the class of [root + preceding frozen adverb, with Proto-Anatolian univerbation] is minimally the presence of such a bi- (ideally tri-) partite contrast (simplex/*pe-/u-* forms). Unbeknownst to me, previous to my establishing these criteria, a storm had already been raging concerning the putative extancy of the simplex *i-* 'give' in a Hittite nominal derivative. Had this much-discussed item eventuated to have any substance, I would withdraw the entirety of my insistence that *pāi-* 'give' is a simplex and no compound. Carruba (1966: 16, fn. 17) – comparing another item – declared:

"Für die Bildung denkt man an die nachträgliche *u*-Thematisierung eines Verbalnomens auf *-u̯ar*, wie etwa in *iu̯aru-* "Gabe, Mitgift", aus einem im Heth. **nur noch in Komposition belegten** **ai-/iia-* "geben", etwa in *pai-* gl. Bdtg." (emphasis mine).

Weitenberg (*Anatolica* 4. 1971/1972: 166–167) goes one step beyond:

"Dieses Verbum [*ai-/iya-] ist nach allgemeiner Ansicht das Simplex des Verbums pai- "geben", das zusammengestellt ist aus dem Präverbium pē- und eben diesem *ai-. (. . .) Diese Etymologie ist attraktiv, (. . .) Es sei aber daraufhingewiesen, daß es andere Etymologien van [sic] pai- gibt: Verwandtschaft mit Toch. B pīto "Verkauf" und ahd. feili "verkäuflich" wird vorgeschlagen (Cop . . .) und Rosenkranz (. . .) erklärt es zu ep-/ap- "nehmen"."[7]

Puhvel (1981a: 213) notes re Carruba's other item: "(It) is derivationally strange and not much helped by Carruba's parallel *iwaru-* 'gift' (alleged **i-war*, verbal noun from the root of *p-ai-, p-iya-* 'give')". It would have helped had Puhvel provided the basis for his "alleged + *" by the simple means of a reference to his 1958 article (reprinted Puhvel 1981b: 46, fn. 2). What Puhvel had concurred with and Weitenberg had rejected was Speiser's (*JAOS* 53. 1935: 436 fn. 17) loan derivation of the Hittite word from Hurrian. Speiser's derivation is surely correct (the Hurrian word-family he associated *iwaru-* with is discussed at length in my article [1982: 20])[8]. Thus, *piya-* 'gives' remains isolated within Anatolian, with almost no basis for being analyzed as *p-iya-*. An examination of the history of the canonization of this segmentation will provide the contextual perspective enabling us to divest ourselves of this enshrined item, the challenging of which verges on heresy.

The earliest etymological speculation is represented by the perplexity of Hrozný (1917: 14, fn. 6):

"Dieses sehr häufige hethitische Verbum (3.P. Sg. Präs. *pa-a-i, pa-iz-zi*), das m.E. etwa die Bedeutungen "ziehen, gehen, fahren; geben, zahlen; nehmen(?)" hat, klingt an ai. *pâti* "hütet, schützt" gr. **πάομαι* "erwerben" an. Wegen der Verschiedenheit der Bedeutungen ist indes ein Zusammenhang sehr fraglich."

The comments (1917: 163) are of great interest and in the *Glossar* (235, fn.1) he has distinguished *pâi* 'er gibt' from *paizzi* 'er geht'. One must needs admire the frank use of the word 'anklingen' vs. (etymological) 'Zusammenhang'. We were still 'pre-preverbial'. The dominance of the preverbial era for this verb begins with Petersen (1933: 32), whose proposal was couched in circumspect word-choice:

"Further evidence of the close relationship of Hittite and Tocharian is the existence in both languages of two common roots which are not found (at least in the same sense) in the rest of the IE territory.[9] . . . The second of these roots is **ai-* 'give', found most clearly in Toch. *e-*, e.g. *e-ṣt* 'thou gavest' . . . In Hittite this **ai-* probably appears in *pe- pai-* 'give', e.g. *pe-hhi* 'I give', . . . inasmuch as the prefix *pe-* . . . was contracted with the root. The development of this root evidently goes hand in hand with the loss in both Hittite and Tocharian of the IE **dā-* 'give', which could not maintain itself because in both languages it had become indistinguishable from **dhā-* 'place', and it was only the latter (in the sense 'take' in Hitt.) that continued to exist."[10]

Pedersen (1938: 115), after proffering the Tocharian-Hittite connection (Götze/Pedersen 1934 with references to earlier works which I have not seen), shortly changed the circumspect 'probably' of his namesake [-voice in the dental] to a definitive 'gewiss':

"Eine weitere diphthongische Wurzel steckt in *pí-iḫ-ḫi* 'ich gebe' . . . Gewiss (WALTER PETERSEN Lg. IX 32) zu tochar. B *ai-tsi* 'geben' mit dem vor einem Konsonanten *pí-e* geschriebenen Präverbium 'hin-'; *pí-iḫ-ḫi* also aus **pe-eḫḫi.*"

Αὐτὸς ἔφα and in the subsequent 45 years few have been those who doubted[11].

2. The Genesis of a Fresh Proposal and the *-K* Factor

The second part of the last sentence of my 1976 article concluded: ". . . else we must throw overboard the idea that *piya-* is a compound.". Thus, in clearing away the rubble of a wobbling building, I raised no new structure to occupy a now vacant lot. Shortly thereafter Warren Cowgill wrote me[12]:

"I have for some time myself believed that these words are not compounds of a preverb 'away' with a root cognate with the Tocharian words for 'give'. One of my reasons is the same as one of yours: the absence of a correlate form with a preverb meaning 'hither' . . . The other is that Anatolian pai-/pi- shows exactly the same vocalism as dai-/ti- 'place', which I am fairly sure comes – directly or indirectly – from *dhoḱ-/dhḱ-, the o-grade and zero-grade respectively of *dheḱ 'put'. This would strongly suggest that the word for 'give' comes from a root *peḱ- or *bheḱ-. But the preverb Hitt. pí-e in my opinion comes from a form (pay or *bay, which monophthongized to pe or be in Common Anatolian when word- or syllable final, but remained with its original vocalism before vowel, as in paanzi from *payanti ← *pay yenti (with the two y's coalescing into one, it seems). And a Tocharian root ay- ought to imply an initial laryngeal, presumably my *x, which ought to appear in Hittite prevocalically as (-h)h-, so that if the communis opinio about Hitt. pai- were correct, the form ought to be rather *pehhai [by "the form" here, Prof. Cowgill obviously intends the form of the Hittite root, not to be confused with *peḫḫi* (1st sg.), where the laryngeal represents the desinence!]. To explain the attested shapes, we'd have to suppose that the root instead had Kuryłowicz's $\underset{\sim}{e}_4$, so that *payai- could merge to *pāi-, and then with loss of distinctive vowel length a new zero-grade be created on the model of dai-/ti- etc. Your point about reduplication being anomalous if the form is really an old compound I had not considered, and is good."

Cowgill's neat equation of the root configurations – within Hittite – of 'place' and 'give' forms the basis for the present proposal. One would like to infer from his analysis that he considers 'give' inherited Indo-Hittite material, but the determination of the Indo-Hittiteness or Indo-Europeanness of a given item, in an Anatolian language or in any single Indo-European Proper language, demands – minimally – something of cognateness in another Indo-Hittite daughter or niece language. I believe that a cognate of Anatolian *pāi-* is present in Indo-European Proper, but in raiment so disguised that (1) our Brugmannian glasses fail to perceive it and (2) the employment of the 'fourth alternative (d)' elucidated on p. 19 above, places it readily at hand. The development of Anatolian 'place' may reconstructed as follows:

o-grade: 3.sg. $*d^{h}oE^{y}e > *d\bar{o}ye > *d\bar{a}ii > d\bar{a}i$
∅-grade: 3.pl. $*d^{h}E^{y}onti > *dyonti > *diyanti > diyanzi$, parallel to which we have the verb 'give':
o-grade: 3.sg. $*b^{h}oE^{y}e > *b\bar{o}ye > *b\bar{a}ii > b\bar{a}i$
∅-grade: 3.pl. $*b^{h}E^{y}onti > *byonti > *biyanti > biyanzi$[13] (cf. on the pre-Hittite reconstructions Puhvel [1960: 55–56] and Kronasser [1956: 165–66 and 197], each of whom conceptualized the glide in "they place" differently, for obvious reasons; my own conceptualization approximates Puhvel's far more than it does Kronasser's).

The Indo-Hittite root $*d^{h}eE^{y}-/d^{h}E^{y}-$ shares a peculiarity with the Indo-Hittite root $*deA^{w}-/dA^{w}-$ (alias **deO-/dO-*) and with the roots **steA-/stA-* and **yeE-/yE-* in that all four show *-k* aorists in Greek (in the active singular). Beyond this, the first and last of these four have this *-k* incorporated into the root in Latin (and sporadically elsewhere – see below). In Anatolian there is no trace of this *-k* **either as a synchronically segmentable morpheme or (even less) as meshed with the root.**

The appearence of this *-k* in the Greek aorists ἔϑηκα, ἔδωκα, ἔστηκα, ἧκα and the root-incorporation of this 'velar 'stem-forming' suffix' in Lat. *facio, iacio* are not to be viewed as dialectal peculiarities. Rather these forms are still visible stages of a continuing process of a major recasting of roots, on-going throughout Indo-European Proper, which (1) would eventually result in neo-roots which are not segmentable into their constituent elements, the second of which is this velar extension, and (2) forms a major heterogloss between the Anatolian continuant of Indo-Hittite and Indo-European Proper. Corollary to this is the heuristic tool, thus gained, of prima facie scepticism of proposals for Anatolian verbs which include such extension (see section 3).

In my article (1981: 959 ff.) I dwelled on the numerous Indo-European Proper neo-roots, constructed of Indo-Hittite √*mei-* + extensions $-\hat{k}/\hat{g}/g^{w}-$ (and $-t^{[h]}-$). The sole datum from that study relevant here is that, in the Indo-European Proper daughter languages, (1) the use of the neo-roots predominates by a large measure over the use of the basic, unextended root and (2) the velar extension apprears in the manifestations [±voice, ±labial component]. In what follows I shall employ the archiphoneme symbol *-K* to denote this unitary set of extensions of 'velar' nature [±voice, ±'palatalization', ±labialization]. The operative model, while founded on the above facts, in no way denies (1) the wide use of other extensions in Indo-European Proper (whether still segmentable or root-incorporated) or (2) the continued survival and use of the unextended 'proto-root' in the Indo-European Proper dialects (differing from dialect to dialect). What the model does demand is simply that (1) the large-scale employment of the neo-roots is an Indo-European Proper innova-

tion and (2) the extensions that coalesce with the proto-root are true consonantal 'suffixes', not laryngeal reflexes or such.[14]

This on-going process in Indo-European Proper can be divided into two major divisions: (1) the one-time extension is already root-incorporated in Indo-European Proper 'proto-times', i.e. for Proto-Indo-European Proper as a whole [old root + extension] already is become [(neo-)root]; (2) the development is accessible, to the surveyor of the Indo-European Proper panorama, as occurring or having occurred either (a) monodialectally or (b) in dialect-clusters. As my prime exhibit, I cite the chestnuts of the Latin, etc. reflexes of the universally accepted neo-roots *$d^{h}ē/ək$- and *$yē/ək$-:

Latin	*făc-io*	(< *$d^{h}ək$- + *yo*)	*fēci* (< *$d^{h}ēk$-)
Umbrian	**façia**	(pres. subj. 3sg.)	**feia** (< *$d^{h}ē[k?]$-)[15]
Oscan	**fakiiad**	(. . . . .)	
Venetic			**vha**χ·s·ϑo[16] (pret. 3rd sg.)
Phrygian	($\alpha\delta$-)$\delta\alpha\kappa\varepsilon\tau$	(pres. 3sg. them., 'afficit')/	$\ddot{\varepsilon}\delta\alpha\varepsilon\varsigma$ (pret. 3rd sg.).

The same zero grade of the neo-root, which is characterized with a *-*yo*-/-*i*- suffix in the present of Latin, etc., – marked with an -*s* – serves as the preterite base in Venetic. In Phrygian the -*k* extended form is apparently specialized for the present, making the apparent similarity of 'sigmoid' preterites in Phrygian and Venetic of minor significance. No cognates of *iacio* are known from the geographically Italic languages (or from Phrygian).

We can speak of *$d^{h}ēk$- as a neo-root in a cluster of Indo-European Proper dialects inasmuch as (1) the position of Venetic as genetically Italic is less than secure[17] and (2) the relationship of even Oscan and Umbrian to Latin as a close-knit Indo-European branch (as e.g. Slavic) is less than one would desire.[18]

Thus, the question of root-incorporation, as a completed phenomenon, remains open even within so-called "Italic". Besides the Umbrian controversy, Venetic has nothing to compare with the form we have. Other Indo-European Proper languages have **stems** of *$d^{h}ē$- and *$yē$- with this -*k* extension, but – where we see it – it **functions**, as we may dare conclude it does in Phrygian (to the extent that we venture to utilize Phrygian forms at all, esp. considering the c.millennium that separates the cited forms). The synchronically non-syrrhizoid Lat. -*do* compounds are another evidence-datum of original non-root-incorporation **for the diachronist only**.[19]

In the case of *$d^{h}ē$-, it is a recurring nightmare of mine to imagine what if we had only these Latin, Oscan, Umbrian, Venetic, and Phrygian + [-*k*] forms with which to reconstruct the root for the proto-language. We would naturally assume an IE (or even worse, IH) √$d^{h}ēk$- or more likely √$d^{h}ak$-. I always wake up from that nightmare to the reality of Gk. $\tau\acute{\iota}\vartheta\eta\mu\iota$, Skt. *dádhāti* and most of all Hittite *dāi*, whereupon I recite the incumbent prayer "Happy is the linguist whose data are not limited." Yet the process could have been inexorable; we could have had

Gk. *$\tau\acute{\iota}\vartheta\varepsilon\kappa\mu\iota$ (**$\tau\acute{\iota}\vartheta\alpha\kappa\mu\iota$)
*$\ddot{\iota}\varepsilon\kappa\mu\iota$ (**$\ddot{\iota}\alpha\kappa\mu\iota$)

or, indeed, *$\vartheta\acute{\varepsilon}\sigma\sigma\omega$ (**$\vartheta\acute{\alpha}\sigma\sigma\omega$) <√*$d^{h}ək$- + -*ye*/*o*- et sim., with
*$\grave{\iota}\acute{\varepsilon}\sigma\sigma\omega$ (**$\grave{\iota}\acute{\alpha}\sigma\sigma\omega$) presential suffix in place of presential reduplication.

Irrespective of my own oneiratic proclivities, this never did come to pass across Indo-European Proper in the so-far cited verbs; any budding linguist, who progresses from Latin

to first year Greek, recognizes that the *-k* is not radical and may even be eager – when he compares the anomalous velar in these four Greek aorists with the *-κ* of the perfect – to extract a "'past' function' as being inherent in 'the morpheme'. So we will have to call a more convincing witness, one whose *-K* has come closer to root-incorporation across the dialect spectrum, one who has experienced the process almost being carried forth to its logical consequence, but not quite: *√yeu-g-*, best rendered by Germ. 'fügen'. As a verbal root – in its prime meanings[20] – it survives unextended only in IndoIranian (e.g. Skt. yaú*ti* besides yu*nák*ti[21]). We can speak here, quite properly, of a verb almost lost to non-extendedness; the linguist who, when he progressed to elementary Greek, recognized the non-radical nature of the velar in *facio/feci: τίθημι/ἔθηκα*, now has both to untertake Sanskrit and become more of a scientific segmentator. It is with a good deal of justice that Pisani (1974) gives the roots as *dhē, dhēk-; i̯ē, i̯ēk*; and I. *i̯eu*, II. *i̯eug*, respectively.

We have sketched so far examples where (1) the *-K* as root-incorporated was limited to a single language or small cluster of languages and its origin as an extension could be readily detected by the addition of multitudinous cognate data (the case of *facio* and *iacio*) and (2) almost all of the data of almost all of the daughter languages would present the *-K* as originally radical except for the addition of the archaic remnant material, limited to one group (the case of *√yeu-* ±*[-g-]*). The comparative method must be built on extant material, but – as a science – is required not to stop with this material; it must and does extrapulate principles and patterns from the data (particularly remnant archaisms) that have been (accidentally) left. It is natural that there are cases of (3): where only the application of the methods, thus gleaned, can recover original relationships in the absence of any extancy of relationships having the neatness of examples of types (1) and (2) above. I have chosen, as the exhibit for this kind of less puissantly apparent relational extancy, a root of the same semantic domaine as the prime topic of this paper. For purposes of summary citation, the forms and definitions are reproduced from Pokorny. He, however, presents what is here conceived as root and [proto-root + extension] having become neo-root, as two separate items with no nexus. Pokorny's 3. *sel-* 'nehmen, ergreifen': Gk. *ἑλεῖν* 'nehmen', *ἕλωρ* 'Beute, Fang'; (base *slē(i)-* cf. Watkins [1962: 179]), O.Ir. *adroilliu* 'verdiene' *(ad-ro-slĭ-)*; O. Icel. *selia*, OE *sellan* 'übergeben, verkaufen'; P's *slăg̑ʰ-* 'fassen, ergreifen': Gk. Hom. *ἔλλαβε* (**ἔσλαβε*) 'nehmen, fassen, ergreifen', Aegin. *λhαβών*, epic. Ion. *λάζομαι* (**λαγ(ʰ)-i̯ω*); "nach *αὖυμαι* ist ion. att. *λάζυμαι*, boöt. *λαδδουσθη* umgebildet"; OE *læccan* 'fassen, ergreifen' (= *λάζομαι* aus **slagʰi̯ō*), NE *latch*[22].

The neo-root(!) **slágʷ-* is built of proto-root **sel-* in theme II: **sl-* + (full grade, *a* variety of) labialized *-K*, as seen e.g. in the neo-root **meígʷ-* (theme I + *-K*). It eventuates that the extancy of apparent relationship is not that much less in (3) than in (1) or (2) and our linguist, who learned Greek and recognized *-K* in *facio*, progressed to Indo-Iranian and uncovered *-K* in *√yeu- ±(-g-)*, should have detected *-K* within the Gk. *ἑλεῖν*: **σλαβεῖν* relationship itself.

All three above cases have *-K* in one or more Indo-European Proper dialect, but not in Common Proto-Indo-European Proper.

I stated earlier (p. 22) that 'alternative d' will unfog our Brugmannian glasses, revealing the IEP cognate of Anat. *pāi-/piya-*. In this Indo-Hittite: Indo-European Proper connection, I propose that we have the following relationships:

IEP neo-roots

Theme I. + *-K-*	Theme II. + *áK-*
$\sqrt{d^h\acute{\bar{e}}}$- (< $*d^heE^{y-}$) ±(-k)	$\sqrt{s(e)l}$- ±(-$ág^{w-}$)
$\sqrt{yeú}$- ±(-g-)	$\sqrt{b^hag}$- (<$\sqrt{b^h[e]E^{y-}}$ +[-ág-]).

It is but one variable that divides $\sqrt{b^hag}$- from the three other exhibits: the segmentation is not perceptible within Indo-European Proper, i.e. the root-incorporation of *-K* is already complete in Proto-Indo-European Proper and our linguist has had to go beyond 'Indo-Greek' to Anatolia. The 'economy' with which the etymology is proposed here is a factor of spatial constraints. The phonological and laryngealogical considerations do, however, perform their constituent roles in the proposal. An overview of the relationships between $*d^h\bar{e}$- and $*b^hag$- may be given in a proportion, combining Cowgill's original terminology and my own: Proto-Indo-European Proper neo-root $*b^hag$- ($*b^h\acute{x}$-ág-): IH $*b^he\acute{x}$- = "Latin" neo-root $*d^h\bar{e}k$- ($*d^h\acute{e}\acute{x}$-k-): IE (and IH) $*d^he\acute{x}$-.

Much as IH morphemic relics continue sporadically in Indo-European Proper (e.g. $\sqrt{yeu}$- ∿ $\sqrt{yeug}$-), so too do semantic archaisms make their way, on occasion, through the 'Indo-Hittite: Indo-European Proper barrier'. As well-known, the semantic opposition partner of Hittite *pāi-* is a cognate of Indo-European Proper $\sqrt{d\bar{o}}$-, a root which in Hittite has the old meaning 'take' vs. the Indo-European Proper meaning 'give'. We have seen that in Greek this is one of those four roots which show the initial stages of a *-K* extended root (aor. ἔδωκα). Just as in *facio* and *iacio* Latin has completed the root incorporation, I believe that one dialect of Indo-European Proper has a neo-root, built of $*d\bar{o}$ + [*-K* root-incorporated], the etymology of which would long ago have been recognized if the semantic variable, fed into etymologizing, did not exclude Anatolian 'divergencies'. This monodialectal Indo-European Proper neo-root $*d\bar{o}g$- (< $*déA^w$-g-, matching Gk. ἔδωκα, but with [+ voice *-K*]) is the etymon of ON pret. *tōk*, while the ϕ-grade $*dəg$- (< $*dA^w$-g-, formally equivalent in struture to the first syllable of Lat. *facio*) yields the ON pres. *taka*, which are used today in English as *took, take* respectively. The model of (1) preservation of archaic meaning and (2) the Indo-European Proper neo-root (attested embryonically for this root!) offers more explicatory puissance than the tenuous connections with the Gothic verb of a different semantic sphere, usually assumed. Solta (1960: 105) assumes the same ablaut for the Armenian cognate: *tam* (< **də-i̯e-mi*), *etu* (< **e-dō-m*), respectively 'I give', 'I gave'. The alignments of the aorist/preterite here is not without interest: Arm. *etu*, Skt. *á-dām* (**e-dōm*): Gk. ἔδωκα, ON *tōk* (**[e]-dō-K-*), no corresponding alignment existing in the present.[23]

3. The Fata Morgana of Anatolian + (*-K*)

A perusal of the entrenched allegations cannot be eschewed, but must be executed in fashion most summary and limited to those germane to the exhibits of this paper: (1) in a drawn-out attack on 'the etymological bases' that Sommer used to understand *takšul* and *takšan*, to wit derivation from $\sqrt{te\hat{k}þ}$-, Pedersen (1938: 139–44) canonized a prima facie improbable derivation of *takšul* (and words which he allowed to be connected) from neo-$\sqrt{d^hək}$- +, while he consigned *takšan* to $\sqrt{dw\bar{o}}$- +: "Allem Anschein nach ist /*dakkeszi*/ mit

lat. *facesso* identisch". Pedersen was overly taken with the Phrygian comparison *αδδακετ/εδαες*, the aorist being a reasonable 'structural' parallel to Hitt. *daiš*. He analyzed *takš*-as having the *-k* seen in Latin and Phrygian and the Hittite stem as a whole as having this same *-k* + the *-s* 'Erweiterung', the 'combination' seen in Ven. **vhaχ·s·ϑo**. Unfortunately, his semantic comparisons are thin; the morphological segmentation is horrible; the phonology leaves much to be desired; and Phrygian is only geographically – not genetically – Anatolian.

Pokorny has the best of all worlds in dividing the meanings under √*d*ʰ*ē*-: "Dieselbe *k*-Erw. . . . auch in . . . (*dakkeszi*) 'macht, stellt hin' . . . (*daksul*) 'freundlich'" and √*tek̂þ*-: "*takš*-, *takkeš*- 'zusammenfügen, unternehmen'." Hrozný (1917: 56, fn.2) had suggested that *takšul* could perhaps belong to the root of Skt. *dákšati*, Lat. *decet* (thus IE √*dek̂*-). For *takšan* Hrozný (1919: 63, fn.9) had suggested "in Reihe", an *-so* stem from the root seen in Gk. *τάξις* (IE **tăg*-, in which he conjoins what Pokorny gives as two roots, *tag*- and *tāg*-). Watkins (1969: 117) construes *takš*- as being an extension within Hittite of *takki*, which had been etymologized by Laroche as < **dok̂*-[24]; this latter is an *o*-grade of the root seen in Gk. *δέκτο* and the extended form (< **dok̂s*-) is reminiscent of Gk. *δόξα* (thus, IE √*dek̂*-). Scholarly decency would have been well served if the Doctor Cambrigiensis had deigned to take note of the proposal of the Pioneer. (2) It was Sturtevant who canonized *tekkuš*- as a Hittite cognate to IE neo-√*deik̂*-.[25] In his article (*Language* 6/3. 1930: 228) he refined his analysis (of *Language* 6/1. 1930: 27) with the following formulation: "Hittite *tekkuš*- . . . represents IH ***deikus*-, i.e. IE **deik̂*-plus the element *u/u̯* . . . plus the formative *s*, which gave rise to the IE present and aorist suffixes in *s*" (thus neo-√*deik̂*- + +, and in *tekkuš(š)-anu*-, we can add another '+'). Feist (s. *ga-teihan*), in citing Sturtevant's analysis, includes an apparently personal communication from E. Forrer: "bezweifelt von E.F.: die Bild. scheint luw.". 44 years later one is still tantalized by Forrer's grounds for perceiving a Luwian structure.

In 1974 Puhvel (reprinted 1981b: 263) uses this word as an exhibit for the Hittite reflex of medial preconsonantal **-k*ʷ⁻: "*tekkuššai*-/dekʷsai-/, *tekkuš(ša)nu*- /dekʷsnu-/ . . . , if cognate with Avest. *daxš*- 'teach', *daxšta*- 'sign, characteristic'. The Avestan words are otherwise isolated (unrelated to Skt. *dáks*- . . . , *daśasyáti* . . . , Lat. *decus*, IE **dek̂*-) but point clearly to a labio-velar . . . , whereas the standard comparison of *tekkuš(š)*- with IE **deyk̂*- . . . must operate with an unexplained suffixal **-us*-." Watkins (1969: 229) speaks of a thematic *-s* suffix to a stem in *-u*, thus **tek-u-se/o*-, from a root **tek*- (seen in Gk. *τέκμαρ/τέκμωρ*) as there is neither phonological nor morphological justification for the traditional comparison with the Greek and Latin *-s* aorists, *ἔδειξα*, *dīxī*.

In utilizing the tradition of *dakkeš*- < **d*ʰ*ək*- +, and *tekkuššai*- < **deik̂*- + +, to buttress her own argumentation of a "coalescence de deux élargissements" in the IE suffix *-sk̂e/o*-, i.e. an agglutination of independent morphemes, allegedly seen in reverse order in *da-k-(e)š*-, etc., my dear friend, F. Bader (1974: 35), is relying – quite irrespective of the merits of the argument as a whole – on the support of the proverbial fragile reed (here not Egypt, but putatively Ḫatti). (3) Hoffner (1977: 105, 107) determined the **meaning** of Hittite *ekt*- as '(hunting) net' and, with Berman, concluded that this item was the Nesite cognate of *aggati*-, "the Luwian word. . . based upon the same Indo-European root (perhaps the verb **yē*- with *-k*- enlargement; cf. Latin *iaciō, iēcī* . . .) . . .". Hamp (1978: 120) laconically noted: "This well known root . . . would not be expected (outside of Latin, where we also find *faciō fēcī*) to show the generalized *-k*-, particularly in such a nominalization . . .

therefore we do better to seek a root terminating in a velar (or Indo-European palatal) (my emphasis). . . . I propose that we now have a new IE root **ieḱ-*, or **ieḱ'-* 'hunt'". In 1972 (reprinted 1981b: 222) Puhvel had likewise (per the ex-meaning of the word, 'leg') proposed a derivation from the neo-*√ey-gh-* (proto-root **ei-*) 'go'.[26] In a neo-footnote (1981: 414, fn.41) he now offers *ekt-* 'net' as cognate with Lat. *ictus*, < **ayk̂-t-*. This is Pokorny's *ăik̂-* : *ĭk̂-* and seems to be the same root as Hamp's **ieḱ'-*, thus **ayk̂-/yek̂-/ik̂-* (I would say **ayek̂-*, but fear to, lest any man segment it as ****ai-* 'give' + *-k̂*, with a semantic development 'give the spear/net to'!).
A chart will summarize the proposals:

Anatolian item:		*tak(ke)š-*	*tekkuš-*	*ekti-/aggati*
Proposed	Alleged Neo-Roots:	***dʰək-*	***deik̂-*	***yēk-*, ***eigh-*
IE	Reasonable proposed √'s	**tek̂ḫ-*	**tek-*	**yek̂-* | **ayek̂-*
Roots:	Real Roots (IH Proto-√'s)	**dek̂-*	**dekʷ-*	**aik̂-* | *(*Aeyek̂-)*

My object in this section is not to weigh the recent reasonable proposals, but only (1) to emphasize that, whatever roots may be involved in the respective Anatolian words, they are certainly not the IEP neo-roots **dʰək-*, **deik̂-*, and **yēk-*, and (2) to demonstrate the debacle effectuated for Anatolian etymology by the non-recognition of the -[-*K*] factor in Anatolian vs. its extancy and productivity in IEP, in whose early stages its *Wuchern* (the connotations of which German "articular infinitive" English cannot express) had its genesis.[27]

Notes

1 The subtitle is, of course, a take-off on "Multiple Stem Conjugation: An Indo-Hittite Isogloss?" by my lamented friends, J. Alexander Kerns and Benjamin Schwartz (1946), *zixrōnām livrāxā*. In citations, every attempt has been made to reproduce the style of the quoted author, e.g. ±italics, ±diacritics, etc.

2 For the terminology and the thereby implied perspective, see Adrados (*JIES* 10/1–2: 1982).

3 See my articles 1974: 70–76 and 1976: 79–80. I am preparing one further article on this verb (with, inter alia, reference to its extra-IH connections), which is already cited in Bomhard (1984: 203).

4 A speculative, but undemonstrable, perspective within this choice could maintain that Anatolian alone preserves so great an archaism of ur-vocabulary that all traces of the item have vanished from all cousin languages. Such reasoning, though possible, leads nowhere.

5 Those of my school would conceptualize this choice as Anatolian having preserved a more primitive (1) configuration (formally) and/or (2) meaning (functionally). This conceptualization could (for him who so wills) be oppositely set, to me an unlikely analysis of relationships.

6 On *pe-* see my view 1974: 71 where one of these reasons is already mentioned. I am grateful to G.J. Ayala for having pointed out to me that that article would have been more logically entitled 'Why Two Adverbs . . . Became Inseparable (Hence, Preverbs)."

7 I have not seen the article cited by Weitenberg, but Rosenkranz repeats the view in his book (1978: 37–38), where "*pai-* 'geben' zu *ep/app-* 'nehmen'" is considered under the heading of quantitative ablaut. We are told that, in the listing of these examples, "seien einige bisher kaum beachtete Bei-

spiele für ein Nebeneinander von Status I und II der Wurzel vorweggenommen, sämtliche Ableitungen mittels des Suffixes *-ai-/-iia-*". Besides the semantic tenuousness of most of the examples, I find the phonological principles beyond my grasp: if the Hittite ablaut *epzi/appanzi* < **H̯ep-/H̯p-* (the second Hittite form < ϕ-grade, with vocalization of the laryngeal vel sim.), can the same ϕ-grade **əpāi-* > *pāi-*? This latter process seems to be that which occurs e.g. in Skt.: *ásti/sánti* < **H̯ésti/H̯sónti*, but precisely that which does not occur in Hittite, where the same reconstructions > *ešzi/ašanzi*. Perhaps Rosenkranz has in mind Gamkrelidze's (1968: 89) metathetic laryngeals: **eH̯p-* > *ep-*, **H̯ēp-* > *app-*, **əpāi-* > *pāi-*, to be sure a variation even on Gamkrelidze's view. Re Čop and other views, see fn. 11.

[8] The rest of the sad tale of the proposals for *iwaru* is well summarized in Tischler (1983:455).

[9] The first alleged isogloss does not concern this paper.

[10] Petersen's preverb-root division seems to leave no root, only preverb (*pe-*) + desinence (*-hhi-*). The history of the proposals re *dāi-* 'place' and *dā-* 'take' is – if possible – more obfuscationistic than the *pāi-* proposals.

[11] To properly understand the context in which **p-√ai-* came to be crowned, a historiography would be necessary. For such (and equally for the matters of fns. 7 and 10), there can be no proper place here. Most of the "doubters" base themselves on the grounds mentioned in fn. 13.

[12] Personal correspondence, dated 26 April 1977. Credit goes to Prof. Cowgill for the framework, but he – of course – bears no responsibility (for worse or for better!) for that which I will here attempt to build on his foundation.

[13] In juxtaposing the same set, Watkins (1962: 100) gives the following chart (relevant for his purposes there, sadly disappointing for understanding Anatolian *pāi-/piya-*):

dheH-s (Skt. *dhās*) : *dheH-* (Skt. *dhā-*)
Hei-s (Hitt. *p-aiš* 'gave') : *Hei-* (Hitt. *p-ai*).

The grounds for dubiety of almost all the doubters are a rehashing of the old debate between Sturtevant and Couvreur concerning Hitt. *a* = IE *a* being a reflex of the ad hoc established 4th laryngeal. Cowgill's objections, thus, accord with (1) this universally repeated caveat and (2) the conjugational parallelism of *pāi-* with *dāi-*, as noted e.g. by Puhvel (1960: 55–56, where he is as 'non-committal' in the opportunity presented for a self-extrication from **p-√ai-* as he charges Risch with being re the *dāi-* type as a whole) and Kuryłowicz (1979: 146).

[14] In **some individual** words and categories we **may** have Verschärfung or other kinds of reflexes of former laryngeals; but the PIH I envision was a whole lot more that the sum of laryngealistic grunts. This PIH (> PIE and Pre-Anatolian) had a morphology, had suffixes, and –as such – was a language, not an assemblage of primaeval hunting calls.

[15] The summary and views of Poultney (1959: 131–32) on the pres. subj. 3sg. *feia* and related forms are critical for our analysis: "These forms have been referred to the full grade of the root **dhē-* . . . , sometimes to the extended **dhē-k* . . . In favor of the second analysis are . . . and the fact that this **root without the *-k* extension is not positively known to have existed in the Italic languages** [in the simplex] ". References are provided to the controversy between Von Planta (< **fē-*) and Buck (< **fē-k-*). Poultney's second support for the < **dhē-k* analysis is, alas, a less than reliable testimony, if we conceptualize the process as productive, alive, and well **throughout** IEP and if we add to this the factors discussed in footnotes 16–19 below.

[16] Beeler (1981: 67): "Another [significant lexical isogloss of Venetic with "Italic"] is the occurrence of the verb **fak-*", citing these Latin, Oscan, and Umbrian forms.

[17] Polomé (1966: 59–76) declares against a close Venetic-Latin genetic relationship, much of his article being devoted to old views of Beeler (adumbrated, on p. 52 in an article on other matters in that same volume, by Beeler, pp. 51–58). In his most recent declaration on the relationship or lack thereof, Beeler (1981: 65–71) comes, after 40 years of struggling with the data, "to the disheartening conclusion that the problem is essentially insoluble. . ."

[18] See Beeler (1966: 51–58) re a languages-in-contact explanation in preference to a Proto-Italic.

[19] The *-do* palaeo-compounds (cf. fn. 15 above) are not relevant synchronically. But beyond this – and more important – it is questionable whether, without the IE comparative materials (esp. the Gk. ± [-κ] **stems**), even the diachronist would have connected these compounds with *facio* rather than with *do* 'give'. There is an important distinction here in the possibilities for the diachronist as opposed to the comparativist, the former being *sensu stricto* scientifically monoglottal. On the native understanding of the palaeo-compounds (< **dʰ[Eʸ]* -), see Ernout-Meillet (1959: 179–80). I find Kronas-

ser's (1956: 166; 197) comparisons of these Latin compounds with putative Hittite compounds, allegedly built to the Hittite cognate of the Latin root, unacceptable (see my article on **d^{h}ē-*, *RHA* 31. 1973: 104). The observation of Ernout-Meillet (1959: 304) that, as opposed to the situation whereby we have palaeo-compounds in *-do* as well as neo-compounds in *-ficio*, in the case of *iacio*, all compounds are of the *con-ficio* type, none of the *con-do* type, is indicative once again of the on-going-ness of the process. The causative factor for the absence of the **con-io* type ‖ *con-do* type is obviously that all one would hear/see is preverb (*con-*) + desinence (*-io*, as in e.g. *capio, audio*); in the important 1sg., there would be no meaning-bearing root.

[20] I cannot here discuss any further sense developments. Suffice it to say that I tend to concur with the further connections considered by Pokorny: 507, 508 & 510, only with a different conceptualization of the interrelationships.

[21] The survival of **yewos* in Lat. *iūs* is a factor of the archaic juridical nature of the item. Concerning the meaning 'the fit/meet' ‖ Skt. *ṛta-*, Hitt. *ārа* et al., see my comments (1981: 906–12 et passim) and (1982: 34–35). There is a similar semantic archaism in German 'mit Fug und Recht'.

[22] The old meaning of Norse *selia*, viz. 'give', survives in Mod. Eng. *handsel* 'a present to express good wishes', originally 'a giving of the hand'. The former meaning of Mod. Eng. *sell* is still seen in the Anglo-Saxon Gospels, e.g. Mark vi:25: *uolo ut* ...des *mihi* ... *caput Iohannis* rendered *Ic wylle* þæt ðu *me* ... sylle *iohannes heafod* (other mss. selle [see the ms. and dialect distinctions in Skeat (1871)]). The competing Germanic lexical item occurs, inter alia, in the Gothic of the same passage: Gk. Θέλω ἵνα ... δῷς μοι ... τὴν κεφαλὴν Ἰωάννου rendered *wiljau ei mis* **gibais** ... *haubiþ Iohannis*.

[23] While concurring for the 3sg. (*et* = Skt. *á-dāt* < **e-dōt*), Godel (1975: 126) insists that the preservation of the presence of a root vowel in the other persons (e.g. Solta's *et***u**) demands the at one time extancy of a subsequent syllable. He excludes any comparison of this lsg. with either Skt. *ádām* or with Gk. ἔδωκα and favors a derivation of *etu* from *[*e-*] *dōsom* (comparing -s aorist forms of OCS: *daxŭ* and of Alb.: *dašë*). Meillet (1936: 133) offers an anomalous preservation < **etuy*. Kerns and Schwartz (1972: 61) offer an overly summary aside: "... a few whole paradigms as sg. 1 *edi, etu* seem recast from IE athem. root aorists **e-dhē-m, *e-dō-m*..." The niceties of the Armenian conjugation as a whole need not concern us here, provided we limit – for present purposes – the comparison to 3sg *et* with Skt. *á-dāt* (IE/IH possibly *[*e-*]*dō-t/s* (Godel 1975: 126).

[24] Note the remarks of and references in Melchert (1977: 102 and 105 with fn.12) concerning (1) Laroche's etymology; (2) connection of Anatolian palaeo-√*dāk-* with Toch. A. *tāsk-*, B. *tās-* (< **tāk-sk-*), 'resemble', and (3) Van Windekens' 1941(!) connecting of the Common Tocharian item with IE √*dek-*.

[25] The present article had been conceived as having two halves, the second of which concerned the Hittite suppletive verb /de-/, /tar-/ 'say, speak'. In the event, the semantic typological data became a tumulus which, D.v., I will present elsewhere. In the meantime, Puhvel (1982: 183, fn.11) has written that the view that Hitt. /de-/ continues the IH proto-root, seen with + [-*K*] extension in IEP neo-√*deik-/ĝ-*, "found its latest avatar in Y. Arbeitman", with reference to my article in *RHA* 31. 1973: 104ff.. We would have in this poetry a literal case of *Et verbum caro factum est* if only *verbum* meant 'verb' rather than 'word'. Suffice it to say here that the correct cognates of Hitt. /tar-/ are given in Pokorny 1959: 1089 (spec. the Balto-Slavic items listed), whilst I now see the IEP cognates of Hitt. /de-/ as consonant with the scheme which has been sketched in this paper. As such, it would seem to require no avatar or other extra-natural means. At all events, flattered as I am by this logo-theology, my frail vessel is unworthy to serve as the transient dwelling of an IH root. It is not lacking in germaneness to note that Puhvel, *lui-même*, has apparently abandoned his adherence to *tekkuš(a)nu-* et al. <(*m.s.* IEP neo-√)**deik-* (his position in 1960: 26). None of us is proposing any connection between Hitt. /de-/ and *tekkuš-*.

[26] The etymology for the meaning 'leg' seems to originate with Kronasser. See the summary in Tischler (1983: 351–52; 349–50!).

[27] It is startling that Georgiev and I have in common but one variable in the respective etymologies we have proposed for Phryg. βεκος 'bread', to wit an IEP neo-√formed with a + [-g]. I interpreted it (1981: 973) as that which The Distributor/Giver (sc. God) distributes/gives us to take/eat (φαγεῖν), 'Distributor, give, eat' all being cognates of βεκος from neo-√*b^{h}ḡ(e)E^{y}-ág-*. Georgiev (1981: 131) analyzes it as cognate to Eng. *bake*, Gk. φώγω, etc. This latter is, likewise, an IEP neo-√, built with a + [-g] to the proto-root **b^{h}ē-* 'to warm', seen in Germanic **bəðam* 'bath.' Puhvel (1979: 213, reprinted in 1981b: 360) notes a Hitt. noun /bita/ (my phonemization) 'gift, grant, allotment', in one

locus: 'he eats [his] bread-allotment (NINDA-*LAM pitta*)'. Puhvel analyzes this noun as < ***piyatta***, nom./acc.pl.nt. From the formal perspective, there is a neat proportion between the verbs proposed respectively by Georgiev and me and their respective nouns:

IEP Neo-√	Proto-Root Noun
$*b^hag$- (< $*b^hE^y$-*ág*-)	$*b^hitóm$ (< $*b^hiyotom$ < $*b^hyotom$ < $*b^hE^yotom$) "Gift/Portion"
$*b^hē/əg$- (< $*b^héE$-g-)	$*b^hətóm$ "Warmth/bath".

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Iranica in Caucasian

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The very large complement of early Iranian loan-words in the Kartvel Georgian language has been collected up to the 12th century by Mzia Andronikašvili (1966). Similarly A. Genko (1930) examined the Iranian words in the Inguš vocabulary. Scattered among the Caucasian languages are other Iranian words which are important for the still scanty early Iranian vocabulary. Some of these correspondent words had been put together in Bailey (1975: 31–35). A similar statement on *č'igni* has been published in Georgian (Bailey 1982: 99–101). Here are six words which are a distinct contribution to Iranica.

1. Kartvel (Georgian) *c'ign-i, c'ignak'-i* 'book' has been often canvassed[1]. But the connected early Iranian has been overlooked.

Iranian has Zoroastian Pahlavi, New Persian *čak* 'document', written both *čk'* and *čγ'*, that is, *čak* and *čaγ*, in Zoroastrian Pahlavi. This word is in the Sasanian Legal Code the *Mātiyān ī hazār dātastān* 'Book of the thousand decisions'[2]. This *čak* was met by C. Bartholomae in his study of the legal text[3], and it is now in the two editions of the text by A. Perikhanian (1973) and M. Macuch (1981).

The Iranian *čak* can be directly from a base *čak-*, as *tak-* 'swift, bold' is directly from *tak-* 'to run' and *tak-* 'be bold'. But the ZorPahl. *čγ* **čaγ* may show rather a base *čag-*, whence in *čak* the final of **čaga-* has been replaced by final *-k*.

The participle to *čag-* is *čagna-*, later *čigna-*, whence **čaγna-*, **čiγna-*. It is attested in a compound with preverb *pati-, pāti-* in Manichean Sogdian *p'tčγnyy* **pātičagnaka-* 'copy'. As a loan-word this had long been known in Hebrew *ptšgn*, and with *pr-* for *pt-* in Aramaic *pršgn*, Syriac *paršagn-ā* rendering LXX *'αντίγραφον* 'copy'. The corresponding Parthian gave Armenian *patčēn*, and the word is in Zor.Pahl. *pačēn* (or *paččēn* 'copy'), *ham-pačēn*, and Pāzand *pačīn* (SGV 16.24).

This *čagna-* is the form of the participle, as *bagna-* is from *bag-* 'to bestow', Sogd. *βγn-* in *prβγn* 'gift'. It has passed to *bigna-* in Old Persian *bigna-* of the names *Bagābigna-* and *Ariabignēs*.

This *čagna-* without *pāti-, pati-*, is preserved in Dido *č'egen* 'book', Veinakh Batsbi *ǰagn, žagn*, Čečen *ǰaina, žaina*, plural *žainaš;* Inguš *žai*, plural *žainaš*.

The replacement of the *čagna* by *čigna-* shown the early change of *ča-* to *či-* as in Avestan *čanah-, činah-* 'joy', and in Old Pers. *činah-* in the name 'Ασπαθίνης. Kartvel Georgian has this *či-* in the word *c'ign-i* 'book' (with *c'i-* from older *č'-*).

2. Kartvel Georgian *nask'v-i* 'knot', verbal *nask'va*, has the *-v-i* common in loan-words, such as, *nadzv-i* 'fir tree', New Pers. *nāz*, Ossetic *nāzu*, Iron *nāz* 'fir' and *näzi*, Iron *näzy* 'pine'; *mogv-i* 'shoe', Zoroastrian Pahl. *mōk*, Abkhaz *a-magu; nest'v-i* 'trumpet', verbal *nest'va, nest'i; nesv-i* 'melon', Ossetic *näsä*, Iron *näs* 'pumpkin', Inguš *nars* 'cucumber' (to Ossetic *nars-* 'to swell').

The Avestan religious technical term *naska-* of the twenty-one sections of the original Avesta was taken over into Zoroastrian Pahl. *nask*, the southern dialect says *našk* (as in Avestan *araska-*, NPers. *arašk* 'envy'). This Kartvel *nask'-* can thus support ghe connexion with the base *nad-* 'to bind' as in Bartholomae (1904).

The name of the *Aragvi* river is already in Strabon, as *Aragos* (gen. 'Αραγοῦ) and *Aragōn* (acc. 'Αραγῶνα).

3. Čečen *varš*, plural *varšaš*, and gen. *varšan* (used also as an adjective) is rendered 'grove, thickets' in the phrase *varšan jistie jolu* (*j* = *i̯*) 'at the edge of a wooded slope' (Jakovlev 1940: 304).

With this *varš* can be set Avestan *varəša-* (glossed by Zoroastrian Pahl. *vēšak* 'a wood') 'some kind of plant' and *varəšaiia-* glossed by Zoroastrian Pahl. *rēšak* 'root', *bun* 'stalk' and *'dwn*, Pāzand *eyvan* 'upper part of a tree', Parsi Sanskrit *skandha-*. In modern Iranian the archaic Yazgulāmī has *warx* 'a herb', and Šuγnī *warx* 'a mountain grass', from **varša-*. From New Persian the AIW quoted *varšān* 'wood pigeon', Syriac *waršān-ā*, Akkadian *amuršanu, uršanu* (*amu-, u-* for foreign *u̯*).

In spite of its ambiguous *-kṣ-* (which corresponds to four Iranian sounds *xš, š, γž* and *ž*) the Old Indian *vṛkṣá-* can be brought in here. The meaning of later Indian is 'tree', but for the Veda the word is also used of the *ándhas-* 'herb' of the soma plant (the original meaning of *andhas* is clear in RV 1.28.7 of the horse's fodder). In RV 10.94.3 *vṛkṣásya śákhām aruṇásya* is 'the *śákhā* (stalk) of the reddish herb' rather than 'tree'. A parallel for an earlier Vedic meaning can be seen in RV 1.182.7 *mṛgaśya patáror* 'of the flying beast', that is 'bird', where later the *mṛga-* is an animal as RV *mṛgó hastí* 'elephant' or 'deer'. In Iranian the *mṛga-* is 'bird' throughout except Wakhī *merg, marg* female 'wild goat'.

Ossetic *bäläsä*, Iron *bäläs* 'tree' is to be taken from **ulāša-* with the vowel shift as in Khotanese Saka *grāma-* 'hot' (beside first component *garma-*), Middle Parthian (Turfan) *gr'b* from *garba-* 'womb, embryo', Middle Persian (Turfan) *hrg *hrāγ* 'tax' with Khotanese Saka *harga-* 'tax', Armenian loan-word *hark*, Kartvel Georgian *xark'-i*, Arab-Pers. *xarāǰ*.

It is possible to bring these various words together to the Proto-Indo-European base *u̯el-* (Pokorny 1139) under *u̯elk̂-*, Avestan *varəsa-* 'hair', Zoroastrian Pahl. *vars*, Armenian loan-word *vars*, New Pers. *gurs*, to Old Slav. *vlasŭ; uelk-* Av. *varəka-* 'leaf', Zoroastrian Pahl. *vark, valg*, New Pers. *barg*, Khotanese Saka *bāggara-*, Sogdian, Parthian *wrkr*; Old Ind. *valká-* 'bark', *válśa-* 'twig, sprout'. To this the enlargement *-s-* gave Iran. *varša-* and *vṛša-* and Old Ind. *vṛkṣá-*.

4. Avar *parss* and *parssi*[4], plural *parssal, parssabi* 'rock' is of valuable support to the Khotanese Saka *pase* 'pieces of rock, stones' in Jātaka-stava 13 v 2 *pase te rriscye gūhaiñä hvastāṃdä* 'they struck sharp cutting stones upon you'. This was missed earlier in my Dictionary[5]. The *pase* 'stones' will be plural to *pasā-* from an a earlier **pa̯sā-* with subscript

hook, from **palsā-* to Proto-Indo-European *pel-*, from *pel-k̂-* (like Khotanese-Saka *pasa-*, older *pąsa-* 'messenger', base *pars-* 'to send out') beside *pel-s-* in Pašto *parša*, Kati (Nūristānī) *parši*, Old Ind. *pāṣ-* in *pāṣyá-*, *pāṣāṇa-*, Old High Germ. *felis*, Old Norse *fjall.*

5. The Iranian base *tap-* 'to strike' is in Zoroastrian Pahl. *tapāh* 'injury', *tapāhēnītan* 'to destroy' (glossed by *kāhēt* 'to reduce'), New Pers. *tabāh*, and in Iranian *tapar* 'axe', Zoroastrian Pahl. *tapar*, Armen. loan-word *tapar*, Middle Persian (Turfan) *tbr*, tapar, New Pers. *tabar*, Čeremis loan-word *taβar*, Slavic: Russ. *topor* 'axe'. Old Slavonic has *tep-* 'to strike'. This *tapar* is formed by suffix *-ar-a-*. A confirmation of this connexion can be seen in the Tabarsarānī *tabuš, tawuš* (*š* and *šu̯*) 'hammer' (the inflected forms *tabšu-* and *tovš-* are cited in Erckert (1895: 912) *šu̯* as *tabšu̯-* which reflects the suffix *-uša-* used of tool and agent. Avestan has *čakuša-* (gen.plural *čakušanąm*) beside *čaku-* in the plural *čakavō* 'hammer, axe' as a weapon, New Pers. *čakuš*. It occurs besides forms with the suffixes *-učа-* and *-uka-*, as in Zoroastrian Pahl. *čkwč *čakuč*, Turk loanwords *čäküč* and *čäkük*. The base is *čak-* as in Ossetic *cāγd* 'struck', *cāxtā* 'he struck', present *-d- cäγdun*. The Ǧuzz Turk *čäkük* is in Al-Kāšγarī. Kartvel Georgian has *č'ak'uč'-i* and Inguš *čaquč*. This same *-uša-* suffix is found in Old Indian in *puruṣa-* (the *adhibhāvayitar* 'secondary nourisher' following *bhāvayitrī* the mother), connected with the base *pār-* 'to nourish'.

The Tabarsarānī has also from the base *bar-, bur-* 'to cut' a word **buruša-* 'shears' preserved as *ubrušu̯*.

The Caucasian *tabuš* confirms the derivation of *tapar* from *tap-* 'to strike' and excludes a metathetic connexion with **paraθu-*, in Ossetic *färät* 'axe', Khotanese Saka *paḍe* 'axes' from **paratu-*.

Notes

[1] G. Deeters (1965: 53–56) did not connect *c'igni* with Iranian, and it was after all not a wandering word without basis. He rightly set aside any connexion with Latin *signum.*

[2] The word *mātiyān* is the Sasanian form of older *m'tgd'n, m'dgd'n *mātakadāna-* in Parthian form, from which the *-akadā-* has been developed to *-iyān-*, as **mātakadāra-* 'steward' has survived in Armenian *matakarar*, Zor. Pahl. *mātiyār (m't'l).*
From *mātiyān* Armenian took *matean*, gen. *mateni*, and thence came Kartvel (Georgian) *mat'ïane.* The same *-iyār* came from *-adāra-* in New Pers. *šahriyār.*

[3] Bartholomae (1904: part 1: 8, 21; part 3: 30; part 4: 10, 13, 17).

[4] Žirkov (1936) has also *parass*, but it is not to be found in Saidov (1967).

[5] For amendment to *the Dictionary of Khotan Saka* see Bailey (1981: 15).

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Prefixal negation of English adjectives: Psycholinguistic dimensions of productivity

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1. Introduction

It is the purpose of this paper to provide some preliminary results of ongoing psycholinguistic experimentation designed to test the productivity of certain negative prefixes in English adjectives. Despite their preliminary nature, it is hoped that the exposure of these data will stimulate comments and criticisms from colleagues in both psycholinguistics and historical linguistics which will help us to refine both the data and the methodology of the inquiry.[1]

To undertake a psycholinguistic study as a means of approaching morphological productivity is, in my opinion, a fitting tribute to one of the most important figures in the history of American linguistics, Henry Hoenigswald. Hoenigswald is a special blend of scientist and philologist which is fast becoming a rarity in our profession, and it is an honor to present this paper to him in recognition of his place in the field of linguistics.

The issues of this study are familiar to historical linguists and to those whose interests lie in the linguistic history of English. Specifically, I am concerned with the productivity of the negative prefixes *un-*, *in-* (*im-*, *il-*, *ir-*), as well as *non-* and, to a lesser extent, *dis-*. As we will see below, *un-*, *in-*, *non-* and *dis-* are the most productive negativizing prefixes in English today.[2] We are investigating not only the productivity of the specific prefixes, but also how the patterns of productivity are determined. That is, how do speakers choose a specific negative prefix for a specific root?

In approaching the question of general productivity we are asking the following three questions:

(1) Are some negative prefixes more productive than others? It has been claimed (Marchand 1969: 169) that *un-* is the strongest negative prefix in English. We are seeking, in part, to confirm or deny this claim (see also Zimmer 1964).

(2) Are the phonological rules which determine whether a prefixed *in-* will be realized as *in-*, *im-*, *il-* or *ir-* (*intolerant, impossible, illogical, irrelevant*) productive rules for native speakers of English, or are they simply relics of Latin morphophonology which survive in English borrowings from Latin? Put another way, is *impossible* a Latin word which has been borrowed whole, or is it an English word formed productively by native English speakers through the application of internalized rules?

When we consider the data below, we realize that we are dealing here with a skewed pattern of apparently unpredictable variants. Such a pattern makes the task of writing a simple rule of nasal assimilation more difficult than it would first appear:

unmotivated	*ummotivated	:	immaterial	*inmaterial
unlucky	*ullucky	:	illogical	*inlogical
enlarge	*ellarge			
unreasonable	*urreasonable	:	irreverent	*inreverent
unpopular	*umpopular	:	impossible	*inpossible
inroad	*irroad			

The factors determining whether the nasal of the prefix will assimilate seems to be sensitive to whether or not the *-n-* in the prefix is derived from Germanic *un-* or Latin *in-*. Synchronically this might be most easily captured by putting the vowel of the prefix (*i-*) or (*u-*) in the environment of the rule, to read something like "*n* becomes *l* when followed by *l* and preceded by *i*," or in more current format, as follows:

$$[+\,\text{nas}] \rightarrow \begin{bmatrix} \alpha\,\text{cor} \\ \beta\,\text{ant} \\ \gamma\,\text{nas} \\ \delta\,\text{cont} \end{bmatrix} \Big/ \; \text{i}\_\_\_+ \; \begin{bmatrix} \alpha\text{cor} \\ \beta\text{ant} \\ \gamma\text{nas} \\ \delta\text{cont} \end{bmatrix}$$

This rule restricts the assimilation to nasals which are preceded by /i/. It works provided that *inroad* is analyzed as a commound with #rather than a sequence of morphemes joined by +. (Special thanks to Carol Anderson for her help with the details of this rule.)

Processes like anticipatory assimilation of nasals are often referred to as 'natural rules' because they involve articulatory ease resulting from the transfer of feature values from one segment to another. But with negative prefixes the naturalness of the rule seems to be determined by something that should play no part in the rule, namely the preceding vowel. This leads us to ask whether the rules of nasal assimilation in English are productive rules or rules which are only apparently productive because of the vast numbers of borrowings from Latin and French which contain the negative prefixes.

(3) As a sequel to the issues raised in (2), we ask the following question: are native speakers of English intuitively aware of a distinction between Latinate (or perhaps simply foreign) vocabulary and native English vocabulary? Do they feel that *possible, tolerant, operative* and *logical*, for example, are fundamentally different from *kind, good, fat*, and *thin*? If speakers can recover such knowledge about the etymology of words, how is it reflected: by their choice of prefixes; of suffixes; by the segmental phonology of the word; the number of syllables; the stress pattern? The possibility that suffixes mark words as Latinate or English, and that this determines the choice of the prefix *un-* or *in-* merits closer inspection. If it is the case that speakers somehow 'look ahead' to the suffix before attaching the prefix, this might account for some of Jespersen's interesting doublets (1942: 468) like *unjust: injustice; uncertain: incertainty; unable: inability*, where the Germanic *un-* occurs on the bare adjectival form and the Latinate *in-* on the derivationally complex form with the Latin suffix. It would also help to explain why some Latinate words with native Germanic verbal suffixes favor *un-* prefixation, while their Latin counterparts favor *in-*: cf. *unanimated: inanimate; unceasing: incessant; ungrateful: ingratitude; undigested: indigestible.* But this solution is surely too simple: cf. *unresponsive, unromantic, unpopular*, etc.

According to Marchand (1969: 168), the Latinate negative prefix *in-* can be considered an independent formative in English by about 1500 A.D. This results, of course, from the

huge number of borrowings with negative prefixes from Latin and French from the earliest times in Old English, and systematically from the 14th to the 19th centuries. Marchand claims (1969: 169) that *un-* has always been stronger than *in-*, and is ousting it more and more. In Early Modern English, *in-* could be prefixed to almost any adjective with a Latin or French basis, and always according to the Latin rules governing assimilation. But such words as *inceremonious, incertain, inchangeable, ingrateful, incomfortable, impleasing* and *impopular* have all given way to *un-* forms. Yet there are also cases where former *un-* words have given way to their earlier etymological negative, viz. *uncredible, undubitable, ineffable, unexcusable, unexpert, unfirm, unformal, unglorious, unperfect, unpiteous, unpossible.* I am unaware of any examples in English of an etymologically native English word which is prefixed with *in-* in any of its purportedly productive forms (cf. Zimmer 1964: 29). Thus, while it is true that some former *un-* words have gone over to *in-*, this has only happened with words of Latin origin. *In-* (*im-, il-, ir-*) is never prefixed to native English words to mean 'not' cf. **imborn* 'not born'; **irrocky* 'not rocky,' **illoved* 'not loved,' etc. If *in-* is indeed an independent formative, why is it only found with Latinate words and not with etymologically English words?

2. The Experiment

We began our investigation of English suffixation and prefixation by studying negative prefixes for one simple reason: while speakers have great difficulty assigning meaning to prefixes and suffixes generally, they are consistent in assigning negative prefixes attached to adjectives the meaning 'not + adj.'.

Several experiments have been designed which I believe will shed some light on the three questions above. I must stress again the preliminary nature of the results, but I hope nonetheless that they will set the basic pattern of investigation in the open. I will report here only on the results of the Antonym Production Survey (APS), which is a production test designed to force subjects to generate what they consider to be the most acceptable opposites to the words they are given.[3] All subjects were sophomore-level undergraduates at The Pennsylvania State University.[4]

The APS utilizes a data list which consists of sixty pseudo-words, thirty of which are Latinate and thirty of which are English (Anglic)[5] in character and appearance. The words were controlled for various phonological and morphological features. The Latinate words were composed of segments, phonotactic patterns and suffixes which made them appear to be unfamiliar Latin words. The same was done for the Anglic list, though it must be admitted that it is an infinitely easier task to devise pseudo-Latin words than pseudo-English ones. The conditions for which we controlled in the design of the words were the following:

(1) The number of syllables
While at first we tried to match Anglic-Latinate words by number of syllables, it became quickly apparent that the Latinate words simply had to be longer in order to appear Latinate. The differences were kept to a minimum by limiting the Latinate words to one syllable more than the corresponding Anglic word (e.g. Lat. *limoral* (3): Eng. *lurmy* (2)).

(2) The suffixes
Latin adjectival suffixes are generally longer and have a more complicated internal structure than their English counterparts. Such Latin suffixes are also quite plentiful in English,

and are easily attached to pseudo-roots. The same procedure of suffix attachment was carried out for pseudo-English adjectival suffixes and roots, but the results are less impressive. The number of native English adjectival suffixes in the language is quite small, and at least on finds it way into the system via the participial ending of the weak verbs in *-ed.*

(3) The initial segments

While the choice of *un-* or *in-* does not appear to be conditioned by the initial phoneme of the root, the exact form of *in-* (*in-, im-, ir-,* or *il-*) most certainly is. Therefore the initial segment of each word had to be carefully prepared. In all, we arrived at the following distribution among our thirty pseudo-Latin words. The pseudo-English (Anglic) group has basically the same distribution with some minor but necessary variations to conform to native phonotactics:

a) Six words beginning with a labial (*m* or *p*).
b) Six words beginning with *l.*
c) Six words beginning with *r.*
d) Twelve words beginning with either a vowel or a 'neutral' consonant; i.e. neither *m, p, l* or *r*, all of which might trigger assimilation.

The lists are:

Latinate

abfunious	retropulous	miniculous	monarial	retulous
lanurous	sempient	nexibular	prosilient	exilar
medable	conable	profinous	repulient	locanous
addorsent	treculant	repucent	lobunious	robulous
neculous	effucent	entulient	resular	fissular
picculent	libular	limoral	exconal	logenic

Anglic

appenish	rotchity	mitchity	mofitted	rimbled
lattished	sontful	neshorly	plinkity	finkled
miskful	eppish	prafilled	rubaded	loofish
arnful	trookish	relshy	lamoted	ruckful
nellful	esheled	eskity	relby	kibful
pelky	liggish	lonky	ertful	lurmy

3. The Antonym Production Survey

As mentioned, this test is designed to force subjects to generate opposites by prefixation. Each one of fifty-one subjects was given the following set of instructions, followed by the thirty Latin and thirty English pseudo-words. (Note that in the instructions to this test no examples of negative prefixes are provided.)

> Instructions to the test:
>
> In English, new words can be created from old words through the use of PREFIXES. For example, *re- (re-apply), pre- (pre-arrange), post- (post-date)* and *sub- (sub-oceanic)* are prefixes which are used to alter the meaning of some base word.

Notice that each prefix in these examples has a specific meaning: *re-* means 'again', *pre-* means 'before', and so on, so that *rearrange* means 'arrange again', *prearrange* means 'arrange beforehand'.

There are also prefixes in English that give words opposite meanings. These are called NEGATIVE prefixes because they mean 'not'. We are interested in finding out which negative prefixes speakers of English actually use. To help us do this, would you please give us the opposites of the words using the negative prefix which sounds best to you for each word. These words happen to be pseudo-words, but most people have no trouble deciding what the best opposite word should be. Of course, there are no right or wrong answers, so just use your intuition about what is best.

(Anglic)

Word	*Opposite*
1. appenish	_____
2. lattished	_____
3. miskful	_____

⋮

(Latinate)

1. abfunious	_____
2. lanurous	_____
3. addorsent	_____

⋮

The results of this test are quite interesting. They are presented below in both tabular and graphic form, with some following discussion. Listed at the top of each category is the initial phoneme of the root according to the conditions mentioned earlier.

Table 1

Prefix	Vowel		Neutral Consonant		Labial		l		r		Total	
	P-L	P-E	P-L	P-E	P-L	P-E	P-L	P-E	P-L	P-E	P-L	P-E
UN	115	127	120	186	132	161	116	160	87	145	570	779
IN (correctly assimilated)	57	36	50	16	39	35	39	29	86	53	271	169
NON	74	60	56	26	70	50	85	52	65	36	350	224
DIS	30	65	37	54	29	32	37	38	36	46	169	235
IN (incorrectly assimilated)	4	7	2	0	10	4	6	8	12	3	34	15
OTHER	17	19	30	32	27	19	23	17	21	27	118	114

P-L = Pseudo-Latinate; P-E = Pseudo-English

Figure 1: Percentage production of negative prefixes

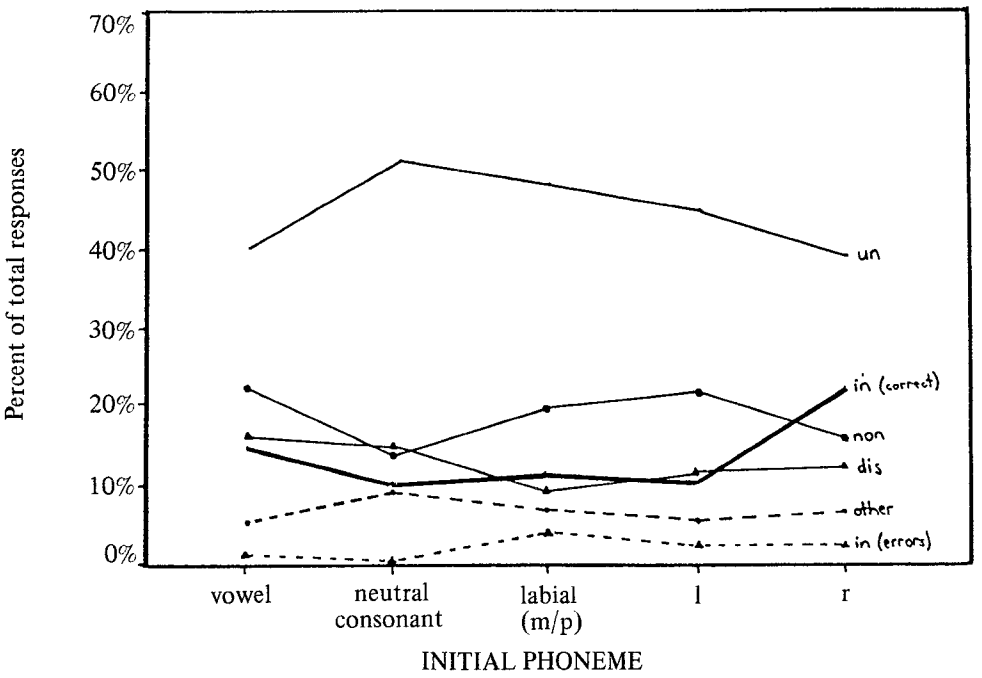
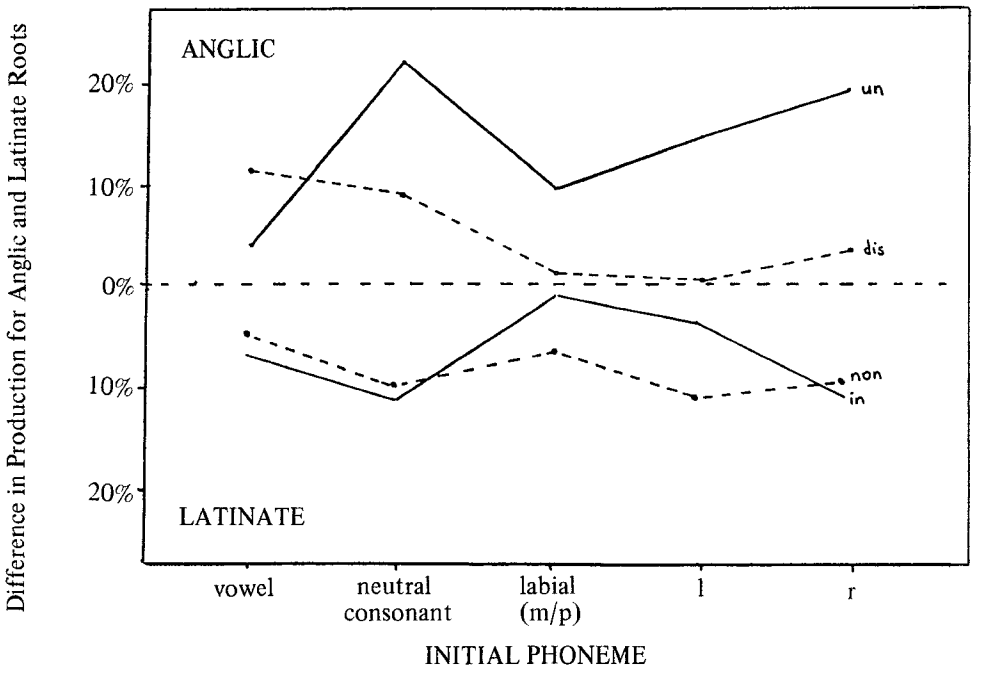


Figure 2: Relative markedness of prefixes for foreignness



From the tabular display and the graphic representations we can discern the following:

(1) *Un-* accounts for 44.3% of the total responses.
(2) *Non-* is the second choice, with 18.8% of the total.
(3) *In-* is third with a total of 16% of the responses; however, when we compare correctly assimilated *in-* (e.g. *irrobulous*) to incorrectly assimilated *in-* (e.g. *ilrobulous*), we see that nearly 90% of the *in-* choices are correctly assimilated.
(4) *Dis-* is the fourth strongest choice, with 13.3% of the total.
(5) All others (*mis-, a-, no-,* etc.) account for only 7.6% of the total.

Compare Figure 1 for graphic representation of the results mentioned in (1)–(5).

Let us look now at the distribution of choices according to the 'etymology' of the root. For graphic representation, see Figure 2.

(6) 61.6% of all *in-* responses were to Latinate roots; while the remaining 38.4% were to Anglic roots.
(7) 42.3% of the *un-* responses were to Latinate roots; while 57.7% were to Anglic roots.
(8) 61% of the *non-* responses were to Latinate roots, while 34% were to Anglic.
(9) 41.8% of the *dis-* responses were to Latinate roots, while a surprising 58.2% of the *dis-* responses were to Anglic roots.

The general conclusions to be drawn from this test are that *un-* is the strongest negativizing prefix productive in English today. *In-* is definitely preferred for Latin-looking roots than for English-looking ones, suggesting that speakers have some intuitive awareness of the foreigness of Latinate vocabulary, though what cues they use is a matter which remains to be decided. It also suggests that the prefixation phonology of *in-* is productive in English. The general selection of *non-* as a second choice is interesting, though once again it is much stronger for Latinate roots than for Anglic ones.

A mild surprise in the data is the preference of *dis-* in Anglic roots to *dis-* in Latinate roots. This results no doubt from the synchronically neutral look and feel of *dis-* which make its Latinate origins opaque. Unlike *a-, in-*, or *non-, dis-* has been completely incorporated into the English lexicon.

Thus the high productivity of *un-* and the low productivity of *in-* for Anglic roots seems to confirm Zimmer's results and Marchand's claims about *un-/in-* productivity.

4. Conclusions and Speculations

It is clear that *un-* is the preferred negativizing prefix in modern English, and that *non-* and *in-* are its closest rivals. It is also clear that speakers have very strong feelings about the relative nativeness of words, and that these feelings guide them in their selection of a negative prefix. What is most interesting is the fact that if *in-* is chosen, it will almost always be correctly used. Thus *in-* seems to be morphologically restricted, but the rules governing its use are phonologically productive. Why these rules have not spread to *un-* is an interesting, if unanswerable question, as is the converse, viz. why hasn't *in-* leveled out its variants to match the pattern of *un-*? Perhaps the *in-* words really don't have two morphemes, and words like *illogical, impossible* and *irreverent* are stored separately in lexical memory from their positive counterparts *logical, possible* and *reverent.* The answer to these questions lies in the future.

Notes

[1] As of this date (May, 1983), two experiments have actually been run, though the complexities of tallying the results leave me with only one to report on in detail here. Two more are being prepared, and will be administered in the coming months. The present paper represents only a fragment of a much larger study being conducted by David Palermo, Victor Broderick and the present author which is designed to test English prefixation and suffixation in general. I hereby acknowledge my indebtedness to these two collaborators, but free them from blame for whatever faults lie in the present paper. Broderick prepared the graphs and helped me with the statistical calculations. I would also like to thank Carol Anderson and Donka Farkas for their stimulating comments.

[2] *Un-, in-, non-* and *dis-* are not, of course, the only prefixes in English with negative force. In fact, English abounds in such prefixes: *a- (an-) (amoral, anorexic); n- (none, neither); no- (no-win situation); mis (misbegotten).* These are all quite marginal and highly marked lexically in comparison with *un-, in-, non-* and even *dis-*, which is itself of marginal productivity.

[3] We have also administered a Multiple Choice Antonym Production Survey in which subjects are asked to list their first and second choices from a register of six potential opposites. This test, which is much more complicated than the Antonym Production Survey, will be discussed in a future paper along with the results of other experiments. Preliminary analysis of the data suggests a confirmation of the results found in the Antonym Production Survey.

[4] The entire question becomes more complicated when we take into account such matters as the educational levels of respondents, their degree of foreign language awareness, their attudes towards 'learned' vocabulary and other factors which influence speakers' judgments and choices.

[5] The term 'Anglic' is used to designate pseudo-words which are supposed to appear to be of native English origin. 'Anglic' is opposed to 'Latinate'.

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Indo-European neuters in *-i*

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0. The aim of this article is (1) to present the evidence for neuter *i*-stems with full inflection and (2) to show that there is no evidence for *i/n*-stems nor (3) for neuters with *-i* in the nominative only (and zero, i.e. root noun, in the other cases).

1. Neuter *i*-stems (with full inflection)

1.0. Handbooks often speak of neuters in *-i*, but mostly this refers to adjectives. It appears that there are hardly any neuter *i*-stems in the Indo-European languages, but that there were in Proto-Indo-European action nouns which were very important in the history of the verb (section 1.9.). We shall present the evidence of the separate languages. This survey does not claim to be exhaustive in details except where indicated.

1.1. Indo-Iranian

1.1.1. Sanskrit

The four *i/n*-neuters are mentioned everywhere; they will be discussed in the next chapter. The handbooks further mention *hárdi*, which belongs to section 3.2., and *várі*, which replaces older *vár* (Wackernagel 1954: 291ff, spec. § 190c; 1957: 131, 145, 160).

I checked the suffixes in *-i* indicated in the reverse index of Grassmann's dictionary (1719–22) and Wackernagel (1954). I found no neuter.

Burrow (1955: 175–177) mentions *śami* and *sṛkvi*, which I cannot find (for the latter cf. Mayrhofer 1976: 554 *srákva-*). That *sáci* 'with' is an old neuter is uncertain. Burrow thinks that some forms in *-ya* continue older neuters in *-i: nábhya-* n. beside *nābhi-* f. (where the long vowel rather points to an original root noun), *mádhya-* (which is certainly of Proto-Indo-European date). On *kravyá-* see section 3.1.

Further he believes that *arcís-*, *rocís-*, *śocís-* were neuters in *-i*. But *roci-*, which he adduces as evidence, "ist jung (Pur., Hariv.) und nicht ganz sicher (PW VI 441)", Mayrhofer (1976: 76). See on these words Wackernagel (1954: 365f.).

1.1.2. Iranian

In Old Persian only *dipi-* 'writing' is in one place supposed to be neuter, but others take it here too as feminine (Brandenstein-Mayrhofer (1964: 116)). The word is a loan from Elamite.

For Avestan neither Bartholomae (1895–1904: §§ 189, 406) nor Reichelt (1967: §§ 303f, 363) mentions a neuter. In the Gatha's proper and the Yasna Haptanhaiti I counted 129 neuters (and 17 doubtful cases), but no *i*-stem. Of the reverse index, Bartholomae (1961) I checked the gender of the forms in *-i* not ending in *-ti*. I counted some 130 words. Two are listed as neuter. *tūiri* 'käsig gewordene Milch, Molke' has been connected with Gr. *tūrós* 'cheese' (because of Myc. $turo_2$ */turjos/* from **turio*-?). There is no good etymology. I don't see on what basis the word is considered a neuter (only *tūiriṇąm* N 66 and 67). The word may be non Indo-European, or it could be the neuter of an adjective.

The other word is *tāyūiri-* 'bread'. Again I don't see how we know that the word (only *tāyūiriṇąm* V 16.7) is neuter. It could be an adjective, as is *xšāudriṇąm*, ibid. (Nor do I understand why Bartholomae refers from the one word to the other and to *taršu-*.) The word has no etymology.

The conclusion is that I find only two words given as neuter, both of which are in fact of unknown gender and which have no good etymology.

1.2. Armenian gives no information as the gender distinction has been given up.

1.3. Balto-Slavic

1.3.1. Baltic

In Lithuanian and Latvian the neuter has disappeared. Old Prussian, which still has neuters, has no evidence for an *i*-stem (Trautmann 1910: 235ff). (*mary* 'hab' Voc. is an *ē*-stem.)

1.3.2. Slavic transfered the *i*-neuters (if there were any) to masculines or feminines (Meillet 1934: 417).

1.4. Tocharian has neuter forms only in the pronouns.

1.5. Hittite

Neuter *i*-stems are frequent. Brosmann (1978) counted 87 of them. He points out that 15% of them are loanwords (because there are good etyma or because they have Hurrian endings). "In view of the large foreign element in Hittite, the scant attestation of Hattic and Hurrian and the lack of an etymology for a majority of the neuter *i*-stems, one can be fairly confident that the actual proportion of such loanwords was considerably greater. . . At the other extreme, evidence concerning words of known original gender inherited from Proto-Indo-European is largely non-existent." Only the suffix *-asti* is clearly Indo-European. *dalugasti* 'length' and *pargasti* 'height' are neuters, *palhasti* is variable. As word with this suffix are always feminine in Slavic, it is supposed (Kronasser 1966: 209) that they became neuter in Hittite. But Kronasser did not give an argument why he rejected Pedersen's view (1938: 35) that the words were originally neuters. This view seems more probable, first, because Slavic does not have any *i*-stem neuters at all, and secondly, because a transition from neuter to feminine seems in general more probable than vice versa. This development may be rather important: first, it would show that Hittite retained neuters where they disappeared elsewhere, and secondly, it might indicate that the *i*-neuters, which were often action nouns (section 1.9.), became feminines in other languages too.

Kronasser (1966: 203) writes about the primary *i*-stems that there are hardly any trustworthy Indo-European connections ("Wortgleichungen, wie bei den *a*-Stämmen, finden sich darunter nicht"). Among the neuters he mentions *lissi* 'liver', for which Schindler (*Sprache* 12, 1966: 77–78) proposed an etymology. He interprets Arm. *leard* as **lis-r-t*, to which *lissi* would be a variant in *-i*. (The root vocalism is not essential here. Nor are *-i/-r* variants probable, but Arm. *-ard* can be analogical.) But it is possible that the Hittite word was an old neuter in *-i*, though it remains possible that the *-i* is an Hittite addition (cf. *meni* n. 'cheek, face'). Schindler thinks an adjective, 'the fat one', is possible, but his connection with Lat. *lardum* etc. should be given up.

The conclusion is that a considerable increase of *i*-neuters was caused by loanwords (cf. on Greek below). We should also bear in mind the notorious productivity of the *i*-stems in Luwian. In Hittite there are 195 c and 87 neuter *i*-stems against 24 c and 17–19 neuter *u*-stems. It is clear that the Proto-Indo-European situation was reversed in Hittite. Though there are no words with an Indo-European etymology, it is quite possible that there was an Indo-European kernel.

1.6. Greek has loanwords like *sínapi, péperi* (Schwyzer 1939: 462). Chantraine (1933: 114) adds *iskhí · osphús* Hesychius. The gloss may be "une graphie tardive, ou un simple faute pour *iskhíon* (ainsi Latte)", Chantraine (1968–1980). Chantraine himself connected the word with *iksús*, which proves non-Indo-European origin for Furnée (1972: 393). Non-Indo-European origin is anyhow probable. *álphi* (see section 2.0.) and *méli* (see section 2.0.) are *t*-stems. *thémis* is "vereinzelt und sekundär neutrum" (Frisk 1972 s.v.). The theory (e.g. Benveniste 1935: 34) that the word was *originally* a neuter **themi, -itos* cannot be proven. It rests partly on the idea that the Sanskrit neuters in *-is* were originally *i*-stems, which is most probably incorrect. If **themi, -itos* were correct, it could also have been **themit*. In the same way *kónis* f. 'dust' would have been a neuter because of Lat. *cinis, -eris*, under the assumption of an *s*-stem **konis-, *kenis-* and behind that a neuter **koni*. If this is correct, it lies far back. Indirect evidence would be *ostéon* and *ósse*, which are discussed below.

The reverse index of Buck-Petersen (1945: 14) states clearly that there are no inherited neuter *i*-stems.

In **Latin** I find only two or three words, *mare, rete* 'net' and *?īle. rete* has no etymology and cannot be used as evidence for a Proto-Indo-European neuter. The first word has been considered to be of non-Indo-European origin by Nehring (1959: 122) because of the *a/o* interchange. The neuter plural *īlia* 'flancs, parties latérales du ventre' has in the singular *īlium, īleum, īle*, which suggest that *īle* < **īli* was the oldest form. It has been connected with ἴλια· μόρια (codd. δῶρα!) γυναικεῖα; ἴλιον· τὸ τῆς γυναικὸς ἐφήβαιον δηλοῖ. καὶ κόσμιον γυναικεῖον παρὰ Κῷοις. (Cf. Pokorny 1959: 499). Indo-European origin is far from certain.

Germanic *i*-neuters are found in Gothic and Old Icelandic (*marisaiws* in Gothic contains the word for 'sea'). Only one word is found in more languages, **mari* 'sea': OHG *meri* (also masculine); in the other languages it has become masculine (*i*-stem) or feminine (stem in *-īn*). Further we find:

OHG *bini* 'bee'; as the *-n-* is a Germanic addition, the *i*-neuter is a Germanic innovation.
quiti, quëti 'Ausspruch'.

OS *bini.*
urlagi 'war'; the word is a Germanic creation; see De Vries (1971: s.v. *oorlog*).
hals-meni 'necklace'; OHG *menni*, OE *mene*, OIc. *men* show that it was a *io*-stem.
land-skepi, friund-skepi etc.; in any case a Germanic creation; see De Vries (1971: s.v. *schap* 2).
OE *spere* 'spear' belongs to Germ. **speru-/sparu-.*

There is no evidence for an Indo-European form. On 'sea' see under Latin.

1.7. Celtic British gave up the neuter.

Old Irish.
Thurneysen gives seven *i*-neuters in his Grammar (1946: 191). Only two or three have an etymology.

guin 'wounding' must be **g^{u}honi.* (Cf. section 1.9.)
muir 'sea' < **mori.*
búaid 'victory' has been compared with *Boudicca* and Germ. *Baudi-hillia* 'Siegeskämpferin', which would point to **bhoudhi-* (Pokorny 1959: 163).
cuirm 'ale' belongs with Gaul. *korma, kourmi.* Further connections are (extremely) uncertain.
druimm 'back' is supposed to be a loan from Welsh (Pokorny 1959: 1075).
graig 'horses', gen. *grega.* Not a loan from Lat. *grex* according to Pokorny (1959: 382).
richiss 'live coals' (see Thurneysen (1946: 191) for the gender) has been connected with Lith. f. pl. *pir̃kšnys*, but Vendryes (1959: R 29) notes that even the Celtic form cannot be reconstructed.

Note that *cuirm* and *druimm* have the *o*-vocalism expected in Proto-Indo-European (as least in one type, see section 1.9.).

1.8. Conclusion

There are two kinds of danger with articles like this. One is that the author cannot resist the temptation to deny every form of evidence. The second is that the nuances disappear when the results are cited ("B. has shown that Proto-Indo-European had no *i*-stem neuters"). Therefore I shall try to be very clear in the conclusion.

Evidence for neuters can be expected from Indo-Iranian, Old Prussian, (Slavic), Hittite, Greek, Latin, Germanic and Celtic. There is no positive direct evidence in Old Prussian (and Slavic). There are hardly any *i*-neuters in Indo-Iranian, Greek, Latin and Germanic, and probably none that is of Proto-Indo-European date. Positive, direct evidence is found in Hittite and Old Irish. In Hittite there was a large non-Indo-European influx. Of Proto-Indo-European date could be the words in *-asti* and *lissi* 'liver'. Whether there are more inherited words should be investigated. In Old Irish *guin* 'wounding' will be old, for other words this is not certain. Doubtful is **mori* 'sea'.

As we find *i*-neuters in two 'extreme' languages (Hittite in time, Old Irish in position), it is possible that they retained *i*-neuters from Proto-Indo-European and that the category was enlarged by loans and new formations but lost the old forms. On the other hand, in Indo-Iranian and Greek they seem to have been lost very early. A certainly old type is discussed in the next section.

1.9. Recently it has been supposed that there were Proto-Indo-European neuters of the type *CoCī* that were very important for the development of the verbal system.

Burrow (1955: 177) thinks that the (3rd sg.) passive aorist type *tā́ri, jáni*, continues a neuter in *-i*. Kortlandt (*IF* 86. 1981: 127) shares the idea (1981: 127). As the type is also found in Avestan (GAv. *vāci, srāvī*), the passive aorist is at least of Proto-Indo-Iranian date. If Kortlandt is right in assuming that the causatives are derivatives of such verbal nouns, the type must be very old. He also assumes (1981: 128, note 1) that such a noun was used in the formation of the Germanic weak preterite (e.g. Gothic 2nd sg. *nasidēs* 'you saved' < **nosi dhēs*) and of the Old Irish denominatives, type *-suidigedar* < **sodi sagitro* (see Thurneysen 1946: § 524).[1] This would mean that this type of *i*-neuter must date back to Proto-Indo-European and lived on into the separate languages, although perhaps already as a fixed (indeclinable) form with restricted use. No doubt OIr. *guin* 'wounding' is a remnant of these verbal nouns (it is isolated in Irish, Thurneysen 1946: 448).

Rather speculative is the following suggestion. The neuter dual ending *-ih*$_1$ (Gr. *ósse* < **ok^{u}ih*$_1$) might have its *i* from the neuters in *-i*. This would prove their former importance in Proto-Indo-European. (Note that du. *-h*$_1$, pl. *-h*$_2$ was a not well marked system.)

2. *i/n*-Neuters

2.0. Four Indo-Iranian neuters have *i/n* inflection. On this basis an *i/n*-inflection for Proto-Indo-European parallel with the *r/n*-stems has been postulated (the first was J. Schmidt 1889: 248ff). I do not think this is correct.

2.1. The evidence outside Indo-Iranian is very untrustworthy. What Benveniste (1935: 6–8) presents "gehört entschieden zu den Teilen der Jagdbeute, die Benveniste aus der vollen Jagdtasche als unrechtmässig erlegt wird ausliefern müssen" as Pedersen (1938: 17 n. 1) said of parts of "das wertvolle *r/n*-Wild".

Passing by Lith. *vãgis* etc., Lat. *axis* etc. (and the word for 'ear', where both Av. *ušibya* and Lith. *ausìs* are innovations), we note that for Skt. *hā́rdi* etc. the Germanic forms do not prove a Proto-Indo-European *n*-stem, and retain only three words: Gr. *álphi* 'barley', Lat. *mel* and Lat. *sal* etc.

For *álphi*, normal plural *álphita*, the Hesych gloss *alíphata· álphita ē áleura* would show an old *n*-stem: it would replace **álphata*. Unnecessary to say that this interpretation, based on a gloss, is not certain enough to prove anything for Proto-Indo-European. Latte corrects the form into **alēphata*; cf. Chantraine (1968–1980: s.v.).

Lat. *mel, mellis* would represent **meli-t, melnes*. But the *n*-stem is not certain. Ernout-Meillet (1959: s.v.) consider *-ln-, -ld-* or "ancienne géminée populaire". Leumann (1977: 213) follows Szemerényi's idea (*KZ* 75. 1958: 183 n. 1) that it is analogic to *fel, fellis* 'bile'. In any case the *n*-stem is found nowhere else. From Hittite *melit* no oblique cases are known, but the adjective *meliddu-* 'sweet' and Luw. *malli*, pl. *mallitinzi* are based upon the form with *t*, so *t*-flection seems probable. Gr. *blíttō* must be based on a stem form **mlit-*, which points to an old inflection **melit, mlit-és*.

'salt'

"Auf Grund von aksl. *slanъ* ('gesalzen'), air. *salann* 'Salz', gr. *hálasin húei* (Suid.) setzt Schmidt (1889: 182) einen obliquen Stamm **sal-n-* neben den nom. **sal-d* oder **sal-i* an, eine Annahme, für die jedenfalls der anscheinend späte griechische Ausdruck keine Stütze bilden kann", says Frisk (1960: 79). The Slavic adjective is of course a *no*-adjective derived from **sol-* (Meillet 1934: 267), the Celtic word is supposed to continue **saleino-* (Pokorny 1959: 878) and does not point to **sal-n-*. The athematic inflection of Greek, Latin (the nom. *sale* is secondary according to Ernout-Meillet) and Slavic (the *i*-stem is shown to be secondary by **sol-no-*) must be old.

The non-Indo-Iranian evidence, then, appears to be non-existent. It should also be observed that Hittite, where we find so many *r/n*-stems, has no *i/n*-stems.

2.2. Sanskrit has four *i/n*-neuters: *ásthi, sákthi* 'thigh', *dádhi* 'sour milk', *ákṣi* 'eye' (Wackernagel 1957: 302–306). However, the Avestan cognate of *ásthi* has the following forms according to Kellens (1974: 336–339) (younger forms in brackets, masculine/feminine forms and *a*-stem forms):

	singular	dual	plural
nom.			*asti*
acc.	*as-ca, (astəm)*	*(asta-ca)*	*asti-ca, (astə̄s-ca, asta-ca)*
gen.	*astō, astas-ca*		*astąm, (astanąm)*
instr.			*azdəbiš, azdibiš* = */azdbiš/*

It is evident that the oldest inflection was that of a root noun, and this must be the Proto-Indo-Iranian inflection, and, we may add confidently, the Proto-Indo-European inflection.

Avestan has one form that suggests an *n*-stem, *astəntāt-* YH 41.3. (First it should be recognized that **ast-tāt-* was difficult. Note further that we would expect */astatāt-/*, cf. GAv. *karapō.tāt- /karpatāt-/*. I think that *-ən-* was taken over from a form like Skt. *astanvánt-* (GAv. has *astvant-*), where *-an-* was regular before *-v-*. If this is correct, it proves the existence of other forms in *-an-*.) Iranian, then, may have had *n*-forms too, but this cannot invalidate the conclusion that the root-inflection was the oldest type.

Gr. *astakós/ost.* 'lobster' is considered to represent *-n-ko-*, but because of the vocalism this is uncertain. The word cannot be a Greek formation, and it is hard to believe that it is of Proto-Indo-European date. The word is probably non-Indo-European. (Cf. Beekes 1969: 51; Furnée 1972: 137, who thinks that *stakhós* is not old. *óstrakon* does not represent **Hostrko-*, or even **Hostrnko-*; nor is *astrágalos* a Proto-Indo-European **Hstr-g-(h$_2$)lo-*.)

Further support could be seen in Venetic, where *ostinobos* (in Latin script) is interpreted as 'ossibus' (Lejeune 1974: 337) and explained from **ost-n-*. But it could also be an adjective in *-tino-* (id. 99). *ostiiako* (olim 'ossuarium') has turned out to be a personal name.

Sákthi has in the Rigveda further an *i*-stem form *sakthyā́* and the *n*-stem *sakthāni*. The Avestan cogante appears in three forms, acc. du. *haxti*, gen. *haxt(a)yå* and *haxta* (see section 3.4.), none of which is an *n*-stem. As the *i*-stem can be explained (e.g. from the dual), we may suppose an original consonant-stem as for *ásthi*, which may be confirmed by *haxta*.

For *dádhi* OPr. *dadan*, which is an *o*-stem neuter, shows that there was no *i/n*-stem.

Du. *akṣī́* shows (cf. the accent) that the word for 'eye' was a consonant stem.

Compounds of all these words are from consonant stems: Skt. *an-asth-á-, -sakth-á-, an-áks-* (Wackernagel 1954: 93, 108f; "aber bei uneigentlicher Bedeutung [that is in younger forms] *-akṣi-, -sakthi-* P. 5.4.113"). Though in itself this is not definite proof for consonant stems, it confirms the other evidence.

2.3. The conclusion is that neither Indo-Iranian nor the other languages have evidence for Proto-Indo-European *i/n*-inflection.

3. *i*/zero-Neuters

3.0. Is there any evidence for neuters that had *-i* in the nominative but no suffix in the oblique cases? We shall first look outside Indo-Iranian.

3.1. It should be remarked in advance that the distinction between *i*/zero neuters and neuters with complete *i*-inflection is difficult. One aspect of the problem is that neuters often occur only in the nominative-accusative singular.

I can find only one form for which *-i* in the nominative has been supposed (and *-n-* in the oblique cases, which, however, proved, improbable, supra section 2.0.), the word for 'salt' (Benveniste 1935: 8). However, Lat. *sale* is secondary, OCS *solь* replaces **sol* as appears from *slanъ* 'salted', Gr. *hali-* in compounds (against *háls*) does not prove a neuter in *-i* (though Chantraine still states this possibility, apparently because Benveniste's authority is still strong; cf. below).

Gr. *ískhi* was discussed above (section 1.6.).

We need not discuss Benveniste's theory (1935: 75ff) of a large scale transference of *i*-neuters to masculine-feminines. The least one can say about it, is that it has not been proven. To posit, e.g. **ikri, *orni* for *íkrion, órnis órneon*[2] is gratuitous. Certainly wrong is it to take *klónion* (a gloss in Hesychius) as a basis for **kloni,* as *klónion* is clearly based on *klónis* (after words like *iskhíon*; the gloss begins with *klónion· iskhíon*), which is feminine, as are the cognates Skt. *śróṇi-*, Av. *sraoni-*, Lat. *clūnis*, W. *clun*; OPr. *slaunis* Vocabul. is not neuter, so probably feminine; OIc. *hlaun* was mostly given as neuter, now mostly as feminine (as far as I know the gender cannot be ascertained for Old Icelandic; as it is at present neuter, this gender was assumed for Old Icelandic too; this is improbable, also because the language has no other *i*-stem neuters). Chantraine (1968–1980) again still posits the possibility of a neuter. Nor is there any reason to posit a **kreuHi* n. for Skt. *kravyám* (*á-kravi-hasta-* may contain **kreuH-* or **kreuHi-*, but it is not necessary to posit a neuter for it), Lith. *kraũjas*, OPr. *krawian*.

3.2. In Indo-Iranian Sanskrit has *hárdi*, the four *i/n*-stems and *vā́ri*. The latter replaces older *vā́r*. This *i*-stem may have developed from an *-i* added to the nominative singular, but this is not certain. In any case the nominative in *-i* was evidently a younger form. There is no reason to suppose that *vā́r* (RV) is a secondary form (as does Wackernagel 1957: 34).

The word for 'heart' is now generally reconstructed as:

k̂ēr̥d	pl.	*k̂er̥d-h₂*?
k̂r̥d-és		*k̂r̥d-óm*
etc.		etc.
(*k̂er̥d-i*?)		

It was recently discussed by Szemerényi (1970), with whom I agree on most points. (I am not convinced that the nominative was **kēr* in Proto-Indo-European. I do not believe in a development **kerd* > **kerr* > **kēr*.) He has definitely refuted the idea, repeated over and over again in all handbooks, that there was an old *i*-stem. In fact many languages prove an old consonant stem (Lith. *širdũ* etc. prove that *širdìs* is recent; and even if we would not have that evidence, *širdìs* cannot be used as proof for an old *i*-stem, as is done so often; recently by Brosman (*JIES* 6. 1978: 98), though he knows Szemerényi's article), and the forms with *-i-* can be explained as younger formations (Gr. *kardía* has a suffix found in other words for parts of the body; OIr *cride* < **krdiom* has nothing surprising; Hitt. *kardias* does not prove an old *i*-stem and there is the regular genitive *kardas*; see also below on *kardias*). The Armenian *i*-stem is not sufficient to prove Proto-Indo-European origin; Szemerényi (1970: 526) reminds that Armenian has more often unoriginal *-i*-stem forms (e.g. from *otn* 'foot').

It is most improbable that an *i*-stem existed by the side of this root formation. In that case most languages would have chosen the *i*-inflection, which does not present the difficulties of the root noun. A full *i*-inflection has been assumed especially to account for Skt. *hṛ́dayam*, LAv. *zərəδaēm*. (The same explanation is often given for Gr. *ostéon*.) Not only is a full *i*-inflection most improbable, even if it had existed it would probably not have given, with thematization, the IIr. *-aya*-suffix, as there is no evidence for such a development (Wackernagel 1954: 213–215; there is evidence for *-ya-* from hysterodynamic *i*-stems, ibid. 807f).

I think the explanation of the forms in *-ayam* is the following. "Auch sonst ist im Veda der NAVSg. n. mancher Stämme durch andere Bildungen vermieden" observes Wackernagel 1957: 32) and gives *hṛ́dayam*,as an example. I think this is the explanation: the form replaces the difficult nominative (**ǵhērd* > **hār*). The form is at least Proto-Indo-Iranian, and as its formation in Indo-Iranian is not understandable (Wackernagel 1954: 213), it seems to be of Proto-Indo-European date. The comparison with Gr. *ostéon* and Lat. *hordeum* seems quite apt: in all instances the old form was a root noun that presented difficulties (specially in the nominative). The explanation of the suffix must be that given by Risch (1974: 132) for *ostéon*: "eigentlich 'Knöchernes'", i.e. *-eios* was the suffix meaning 'belonging to', well known from the adjectives indicating materials. The substantivized form of this adjective would get almost the same meaning as the noun from which it was derived. This explanation is also given by Szemerényi (1970: 525), who gives the curious parallels that Lat. *cuprum, fāgus* were in the Romance languages replaced by *cupreum, fāgeus*. I wonder whether the Hittite genitive *kardias* is not simply this adjective, *krdeios* 'belonging to the heart, of the heart'. Compare the Luwian adjectives in *-assi-* replacing the genitive.[3] (A secondary formation on the basis of the dative and instrumental (*kartit*) seems improbable to me: these starting points for *i*-inflection were present with all consonant stems; and why would only the genitive shift to the *i*-stems?) I would even consider the possibility that *hastiias* < **HostH-eios* was the origin for the rather enigmatic inflection *hastai, hastiias*. (Once the adjective was incorporated as a genitive, apparently an *i*-stem form, analogic spread of an *i*-stem was only to be expected.)

The conclusion is that the *-i* of *hā́rdi* is not cognate with the *i*'s of the words for 'heart' in other languages nor with that of *hṛ́dayam*. Proto-Indo-Iranian inherited **k̂ērd *k̂rdés* with an adjective **k̂rdeios* of which the neuter **k̂rdeiom* could be used in the place of **k̂ērd*. The question remains from where the *-i* came.

Szemerényi (1970: 526) assumes that the *-i* developed from a prop vowel before words with initial consonant. He gives parallels of languages that have such (final) vowels. But here the difficulty of this solution becomes clear: some languages have such phenomena, but then rather frequently, others do not have it. And as Sanskrit has no evidence for such a development, this explanation cannot be accepted.

I can think of only one source, which to my mind is evident: *hā́rdi hṛdás* was formed after *ásthi *asthás*. Essential is that the *-i* of *ásthi* can be explained. Note that *hā́rdi* and *ásthi* belong to the same semantic sphere.

3.3. Remains the group *ásthi *asthás*. The *-i* can hardly date from Proto-Indo-European, as there is no evidence for *i*/zero neuters elsewhere. The other languages give no support for a nominative in *-i* in this case. Hitt. *hastai, -iias* does not have *-i*. Gr. *ostéon* has been explained above (from an *eio*-adjective). Arm. *oskr* < **ostuer* and Lat. *os, ossis* have no trace of an *-i*. When the *-i* is of Indian origin, it cannot have been taken from *hā́rdi*, where it must be explained itself, and there were no other neuters in *-i*. The *-i* must have originated in the word itself. The explanation has been given by Hamp (1953: 137–141). It may be well to draw attention to his explanation, as it is not mentioned in Mayrhofer's dictionary (1953: 67, 553; 1976: 637f). The *-th-* shows that the word had a laryngeal, and **HóstH, HostH-és* gave **ásti, *asthás*, with generalization of the *-th-* in Sanskrit *ásthi* (cf. *pánthāḥ*). I arrived at the same conclusion independently. (Szemerényi (1970: 526, n. 61) does not accept the laryngeal, but without any argument. Kellens (1974: 336) says that Gr. *ostéon* disproves a laryngeal, which is not true. Note that Hittite and Greek would have got **hasta* and **oste/a/o* respectively in the nominative.)

The development to **asti* was Proto-Indo-Iranian (Beekes, *IIJ* 23. 1981: 275–287). *hā́rdi* occurs also in Dard- and Kafir languages (Mayrhofer 1976: 605), which means that *hā́rdi* probably existed already in Proto-Indo-Iranian. If *hā́rdi* got its *-i* from **asti*, this must be of Proto-Indo-Iranian date, too. (Cf. below on possible Iranian **dadi*, section 3.5.).

Avestan nom. sg. *as-ca* instead of **asti* then presents a problem. Hamp here makes a suggestion which is apt to discredit his theory: that Av. **ast* represents **Host*, supposing that *-H* was a collective suffix. This is unacceptable. First, Sanskrit and Avestan must be derived from the same form. (And if we assume **Host* for Sanskrit, we cannot explain its *-i*.) Secondly, a collective suffix for (the singular of) 'bone' is quite improbable. We must look for another explanation. (Note that those who assume Proto-Indo-European **HostHi* have the same problem.) I see three possible explanations (which do not exclude one another):

(1) In derivations (*astvant-*), in compounds and before clitics (*as-ca!*) the *-H* was not vocalized in Iranian (as normally in interior syllable).
(2) The inflection **asti, astas* could lead to a new nominative **ast* (a tendency strengthened by the development in 1).
(3) The plural, which was rather frequent given the meaning of the word, was *asti* (< **HostHI*). To avoid the homonymy with the plural the singular could have been reshaped to **ast*. I think, then, that *as(t)* replaces **asti*.

Whether **HostH* originally contained a suffix *-tH* cannot be made out. The comparison with *sákthi* suggests it, but this word could have been assimilated to *ásthi*.

3.4. Skt. *sákthi* 'thigh' may have *-i* < *-H* just as *ásthi*. It is of course possible that it got its *-th-* and/or *-i* from *ásthi*, but there is no necessity to assume this. In any case *śúpti-* 'shoulder' did not take over *-th-*. The *n-* and *i*-inflection (du. *sakthyà*, probably for original **sakthī*) point to a root noun.

We do not know what the nominative singular was in Avestan. The gen. pl. *haxt(a)yå /haxtiāh/* is an *i*-stem. This form may be analogic after the nominative dual in *-i*, but it might also be analogic after the nominative singular (cf. GAv. *jəənayō* 'women' from *jani-* < **g^{w}enH*). Of the form *haxta* (F 3 g *'haxt'*) we do not know which case it was. I see four possibilities:

(1) An *n*-stem nominative is improbable.
(2) A locative (singular) of an *i*-stem is not very probable for this word.
(3) A good possibility seems to be that it is the instrumental singular of a consonant stem.
(4) Lastly, it might be the nominative dual of an *a*-stem (cf. the *a*-stem forms of *ast-*).

GAv. *haxt(i)-* does not agree with *sákthi*: we expect **sakth-* > **haxəδ-*. Again it is possible that the *-t-* was taken over from Av. *ast-* (with regular *sth* > *st*). But in the nominative **haxti* is regular from **sakti* < **saktH*. Compare for the non-generalization of *-th-* in Av. *pantå pantānəm* against Skt. *pánthāḥ* (and, more generally, *aogō aojah-* against Skt. *ójas*). However, in this case we find *-t-* in the oblique cases. This can be explained if the *i*-stem originating in the nominative was generalized. Lastly, the *-t-* of the nominative might have spread to the other cases after the example of **asti*.

3.5. With *dádhi, dadhnás* have been connected OPr. *dadan* 'milk' Voc. and Alb. *djathë. dadan* is probably an *o*-stem. (The only neuter *n*-stem in Old Prussian is *semen*; cf. also Trautmann (1910: § 157b) on *wundan*.) The Albanian word represents **dedh-*. This gives a problem for OPr. *-a-*. Therefore, Szemerényi (*KZ* 73. 1958: 81 n. 5) suggested that *dadan* is a loan from a Germanic word for milk, comparing it with Gothic *daddjan* 'suckle'. I think Toporov (1975: 284–286) is right in objecting: (1) that we have no other evidence for this Germanic word; (2) that the Slavs took their word for milk from Germanic, but that was **melko-*, not **dada-*; (3) that *dada-* occurs in Prussian geographical names. The last point seems not certain to me, the first two, however, make the suggestion improbable.

I see three possibilities for OPr. *-a-*:

(1) There was assimilation **deda-* > *dada-*, but this is ad hoc.
(2) In the vocabulary *a* often is found for older *e* (Trautmann 1910: § 11c). It is a peculiarity of that dialect of Old Prussian.
(3) The original inflection of the word had *o/e*-ablaut. (A fourth possibility is that Alb. **e* is not original.)

The difficulty, then, is not strong enough to deny the at first sight evident relation of *dadan* with *dádhi*. I think there are two more arguments in favour. We now know that (if the word was inherited) it must have had *-dh-*, for Proto-Indo-European *-d-* would have made the preceding vowel long according to Winter's Law. Further, another gloss (690) has *ructan dadan* 'sour milk', which shows that this word could be used in a phrase with exactly the same meaning as *dádhi*.

Therefore the cognates must be accepted. This means that the word did not have *n*-stem forms originally. But an original *i*-inflection as is usually assumed is equally impossible.

In Sanskrit the *i*-inflection might have been replaced by the *i/n*-inflection, (though I think such a development is improbable), but the absence of an *i*-stem in Albanian and Old Prussian cannot be explained.

The word is mostly derived from **dheh$_1$-*, **dheh$_1$i-* 'suckle' with reduplication. But a preform **dhe-dhh$_1$i* or **dhe-dhh$_1$-i* is impossible, because this would have given a full *i*-inflection. Hamp therefore rejected the connection with this root altogether, but this is not probable. See the parallels given by Toporov (1975). The only possibility left is that the root had the *i*-less form and no suffix *-i* (neither throughout nor in the nominative only, for this would certainly have led to *i*-inflection in some of the languages). We thus arrive at the reconstruction **dhé-dhh$_1$*, gen. **dhedhh$_1$-és*, which gives directly (Proto-Indo-Iranian) **dadhi, *dadhás.* The fact that the laryngeal of the root explains the *i*/zero inflection of Sanskrit directly can hardly be a coincidence. The reduplicated root noun might surprise. Kortlandt points out to me that the word for 'beaver' may have had this formation. We find **bhebhro-* and **bhebhru-* (Avestan has an *i*-stem) side by side, and this suggests that the word was simply **bhebhr* originally.

Interesting is Szemerényi's theory that Hungarian *tej* 'milk' derives from an Ir. **dadi* (apud Altheim 1951: 77f). It would point to an Iranian form in *-i.*

3.6. About *ákṣi* we must be short. We have seen that *ákṣi *akṣás* probably was the original inflection. The structure of the word is totally unclear, other languages pointing to simple **Hok^{u}-*. It has been assumed that the *-i* originated from the (frequent) dual *akṣī.* I would expect a complete *i*-inflection in that case, but it is not impossible. On the other hand, a form in *-H* cannot be ruled out either.

3.7. The results for Proto-Indo-European may be given in this table:

neuters in -i	*i/n*	*i/zero*
**ToRi*: IIr. pass. aor.	none	none
Germ. pret.		(**HostH* > PII **ásti*
OIr. denomin.		**HostH-és* > **asthás*)
Skt. – Gr. –		
Av. –[1] Lat. –[2]		
OPr. – Gm. –[3]		
Hitt. + OIr. +		

1) two forms given uncertain; 2) two or three words; 3) recent forms.

Notes

1 Proterodynamic **k^{u}ori, *k^{u}r-ei-*, younger **k^{u}orei-*, would explain the zero and *o*-vocalism of the *eie*-verbs.

2 Hitt. *haran-* shows that the word was originally an *n*-stem. (The nominative *harās* continues **Horō* < **Horōn*, as Kammenhuber (1969: 289) has shown, (note that Hittite and Palaic write *harās* with long *a*); not mentioned by Tischler (1977) who refers to the theory that the nominative had **-ans*, which would have given *-anz*.) Benveniste's **or- *oren-* (1935: 24) thus loses its basis. (Tischler wrongly cites **oren-, *orn-*.) Gr. *órneon* may have the same suffix *-eio-* as has *ostéon* (see the text, below). There is no reason why Gr. *órnis* would be an old *i*-stem. Benveniste made the mistake to assume that *-eio-* requires the former existence of an *i*-stem.

3 Comparable is Szemerényi's explanation of Gr. *gunaikós* as an original adjective (*AION* 2. 1960: 13–30).

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Four contributions to Sanskrit etymology

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1. *kṝ-, kiráti* 'to scatter'

The usual etymology offered for this Sanskrit verb derives it from an Indo-European root *(s)ker-* 'to spring, jump', from which are also drived Gk. σκλίρω 'to skip, dance, frisk' and a variety of other words in various languages listed by J. Pokorny 1959: 933–934). The meanings do not correspond, which renders this proposal uncertain. This derivation also assumes that the final vowel of the Indo-European root involved was *-r*, which cannot be taken for granted. It may just as well have been *-l*. Sanskrit *r* may be derived from Indo-European *r*, or from Indo-European *l*. On the other hand Sanskrit *l* does not, as commonly assumed, freely develop from Indo-European *r*. Where *l* occurs in Sanskrit, whether replacing an *r* in the Ṛgvedic language, or without such an alternative, it corresponds, with few exceptions, to Indo-European *l*. Although the verb *kṝ-, kiráti* only shows *r* in all stages of the language, there are a number of words which have been held to be derived from it, or connected with it, which have *l*. If these connections are justified, then the great probability is that the Sanskrit root *kṝ-* is derived from an Indo-European root ending in *-l*.

In the first place there are the adjectives *ākula-*, 'confused; filled with, crowded (also *vyākula-, samākula-*)' and *saṃkula-* 'crowded together, dense, disordered'. The Petersburg dictionary derived these words from the root *kṝ-*, and the meanings are eminently suitable. Mayrhofer (1953–1980: s.v. *ākula-*) rejected this etymology in favour of a derivation from Proto-Munda which probably he would not now be prepared to consider. On the other hand, under *saṃkula-* he is prepared to accept it, although ready to assume a development of *r* to *l*, which, as noted above, is not acceptable. This matter will be taken up later. In the meantime we may note the point made by Professor Turner (1966: 45), that there is a remarkable parallelism between *ākula-, vyākula-, saṃkula-, samākula-* and *ākīrṇa-, vyākīrṇa-, samkīrṇa-, samākīrṇa-*, the two sets having the same meaning.

The Petersburg dictionary also derived the adjective *kalila-* from this root. The word is used in the sense of 'dense with, filled with' at the end of compounds, frequently with the idea of a confused mass: *rathanāgāśvakalilā (vāhinī)*, etc. In such cases we could substitute *ākula-, ākīrṇa-*, etc. without changing the meaning, and this confirms the derivation.

In addition to the derivatives already suggested we may further connect with the root *kṝ-* another word showing *-l-*, namely the tenth class verb *kālayati* 'puts to rout, chases, pursues'. This verb is used frequently in the epic language in connection with defeated enemies, and the basic meaning is 'scatter, disperse'. It provides further evidence that the *kṝ-* has an *r* corresponding to Indo-European *l*.

Finally there is the peculiar combination of words *kiri-* and *kiṭi-* both meaning 'hog'. There is no doubt about the derivation of the first of these. The root *kṝ-* is used of animals digging up and disturbing earth, and the stem *kiri-* is a straightforward derivative of this, descriptive of the activity of the hog. The same stem as an action noun occurs in the compound *ākhu-kirí-* 'molehill'. The problem is to find out how the form *kiṭi-* can be connected with *kiri-*. This can be done if we assume that the Indo-European root originally terminated in *-l*. In that case *kiṭi-* can be derived from **kilti-*, with the suffix *-ti* added to the *l*-form of the root, and this by the operation of Fortunatov's law would regularly become *kiṭi-*.

In considering the etymology of this root we also have to take into account those forms of it which appear with an initial *s*-mobile. In such cases the form with initial *s-* is usually more original, and the form without the *s-* is to be regarded as having lost it; *paśyati*: Lat. *specio; tanyatú-: stanayitnú-*, etc. So we must look for comparisons in Indo-European having initial *sk-*. This of course has been done, but the error has been to assume that the final consonant of the Indo-European root was *-r*. As we have seen there is plenty of evidence from Sanskrit itself to show that the original root must have terminated in *-l*.

The forms of this root with initial *sk-* occur in connection with certain prepositions. These are as follows:

(1) *apa:*
The examples are from Pāṇini 6.1.142 where the combination *apaskirate* describes the actions of a bull turning up earth with its horns, of a fowl scratching the earth looking for food, and the action of a dog preparing to lie down. As these actions are looked upon without regard to any particular object, the verb is conjugated in the middle: *apaskirate vṛṣabho hṛṣṭaḥ, apaskirate kukkuṭo bhakṣārthī, apaskirate śvā āśrayārthī.* There is also a noun *apaskara-* 'dung'.

(2) *ava:*
The only verbal example of this combination is *avcaskarire* in Śiśupālavadha 5.63. This also depends on Pāṇini's rule 6.1.142, and its meaning is the same as that of the combination with *apa* (subject *mahokṣāḥ*). In addition there is a noun *avaskara-* with the same meaning as that of *apaskara-*, 'dung; a place where dung and other rubbish is deposited'.

(3) *upa:*
Initial *sk-* of this root is again attested only in Pāṇini's grammar, 6.1.141–142. The examples are *upaskāram madrakā lunanti*, the precise meaning of which is not certain, although the commentary renders *upaskāram* by *vikṣipya*, and the participle *upaskīrṇam*, which is also obscure, the commentary paraphrasing it as *vikṣepa-*.

(4) *prati:*
This combination is also known only from Pāṇini (6.1.141), and the meaning is equally obscure. Again the commentary renders *pratiskīrṇam* by *vikṣepa-*.

(5) *vi-:*
This combination occurs only in the nominal stem *viṣkira-* 'scatterer', i.e. a 'gallinaceous bird'.

On the basis of the evidence from Sanskrit so far provided we must look for an Indo-European root beginning with *sk-* and terminating in *-l*.

In Iranian this root is represented by Av. *kārayeiti*, where it has been specialised in the sense of scattering or sowing seed, a meaning also found in Khotanese and elsewhere (Bailey 1979a: 53). The verbal form of Avestan corresponds to that of Skt. *kālayati* cited above. A more general meaning is found in Ossetic *kalyn* 'pour, scatter'. The fact that Ossetic has *-l-* in this word may be of significance, although it is possible to account for this by development within Ossetic itself (Abaev 1958: 570).

A form with initial *sk-* may possibly be attested in Iranian by the Avestan proper name *skārayaṭ.raθa-* which may have meant 'scattering the chariots (of the enemy)'.

A verbal root having a comparable meaning, and a phonetic form corresponding to what is required by the above evidence, is found in Greek: σκάλλω 'stir up earth, hoe', σκαλεία 'hoeing', σκαλεύω 'stir, poke; (of poultry) scratch', σκαλλίζω 'hoe', σκάλσις 'hoeing'. There has been some restriction of the meaning in Greek, so that the use of this verb is confined to the activity of scattering and disturbing earth, but it is to be noted that this precise sense is also prominent in Sanskrit, as illustrated in the examples quoted above.

The same root, Indo-European *skal-*, appears also in Celtic: Irish *scailid* 'scatter, disperse' with a meaning corresponding exactly to that of Skt. *kiráti*. In the dialects of northern England and Scotland there is a common verb *scale* having the same meaning. According to Wright (1855–1930: 570) its meanings are as follows: 'to spread hay or manure over a field, to scatter and level molehills, to dress ground by raking and hoeing, to stir, poke, rake out a fire, to disperse, separate (gathering of people transitive and intransitive); to spill, upset'. This word appears to have no relatives in other Germanic languages and so could possibly be a loanword from Celtic, but whether this is so or whether it is a Germanic word preserved only in these dialects of English, there is no doubt that it belongs to the Indo-European root *skal-*, as represented in the Sanskrit, Greek and Celtic words listed above.

The etymologists have usually connected the Greek word with the Indo-European *skel-* 'to split', which occurs in such forms as Lith. *skeliù* 'to split', OIsl. *skilja*, Arm. *çelum* 'id.', etc. This is not suitable semantically, since 'split, cleave' and 'scatter, disturb, disperse' are quite different meanings. The connection is also unsuitable phonetically, since the forms of the verb meaning 'scatter' unaminously point to an Indo-European root having the vowel *-a-*, i.e. *skal-*. The two groups should therefore be kept separate.

The Celtic forms of the verb meaning 'to scatter' in the Britannic section show some complications: Welsh *chwalu* 'to scatter', Cornish *skulye*, Breton *skulá* 'id.'. The initial *ch-* of the Welsh word is explained as the result of the metathesis of initial *sk-* to *ks-*, of which there are other examples, but the following *-w-* is a problem. J. Morris-Jones (1913: 159) assumed two original forms of this root, *sq^{u}el-* and *sqel-* to account for this development in Welsh, and though he has not been followed in this it seems that some kind of explanation on these lines should be necessary. Of course he assumed the usual theory that the Indo-European root in question was *skel- (sqel-)*, which as we have seen is not acceptable, but a similar variation with an alternative *skal-* or *sk^{w}al-* would account for this development. In this case the Cornish and Breton forms of this verb could be explained as having the weak form of the root *sk^{w}al-* (alternation *sk^{w}al- : skul-*). The same development can also be seen in Gk. σκύλλω 'to be dishevelled (hair), be torn to pieces of dead bodies (medio-passive); (transitive active) maltreat, molest, trouble'. This verb has usually been considered to be connected with σκάλλω (Frisk 1970: 742), and its phonetic form agrees with the above mentioned Celtic forms. This takes back the alternation to an early period.

If such an alternation could occur with the root beginning with *s-*, it could also occur with the alternative form without *s-*; i.e. *kwal-/kul-*. Of these the former is not attested, but the weak form *kul-* can be seen in Skt. *ākula-* and *saṃkula-*, and this would account for the unexpected vowel *-u-* contrasting with the *-i-* of *kirāti*.

2. *chaṭā* 'mass, lump, assemblage'

This word appears late in Sanskrit being first recorded in Śiśupālavadha 1, 47. There is another meaning, 'collection of rays, lustre', but this is secondary arising from such compounds as *kiraṇacchaṭā* 'mass of rays' and *vidyucchaṭā* 'mass of lightning'. Prakrit has a corresponding word *chaḍā* 'mass, assemblage', but *chaḍā* 'lightning' given by the Deśīnamālā is again a secondary development arising from the compound *vidyucchaṭā*. In Buddhist Sanskrit there is a reduplicated form *chaṭa-chaṭā* or *chaṭā-chaṭā* used adverbially in the form *chaṭachaṭāya* or *chaṭāchaṭāye* meaning 'in one great mass'.

The fact that the word occurs late in Sanskrit does not prevent it from having an Indo-European etymology. Such words may have been preserved in vernacular Indo-Aryan from early times, and eventually been adopted into Sanskrit from Prakrit at a comparatively late date. In looking for an etymology of the word *chaṭā* we have also now to bear in mind the fact that intervocalic *-ṭ-* may by the process of spontaneous retroflexion represent Indo-European dental *-t-*, as I have illustrated in detail elsewhere (Burrow 1971: 538–559). Since initial Sanskrit *ch-* corresponds to Indo-European *sk̂-*, we may posit in this case an Indo-European *sk̂at-*, and this will correspond to the *scat-* of Lat. *scateo*, of which the meanings are given as 'to gush, well, spring or flow forth (of wells, springs, fountains)', and also in a more general sense as 'to abound, be abundant, abound with'. It has been generally taken for granted that of these two meanings the first is original, while the more general meaning is a secondary development. The Sanskrit comparison would suggest that the opposite is the case, and this is confirmed by other forms in other Indo-European languages which have been compared with Lat. *scateo* (J. Pokorny 1959: 950). Thus Irish *scatán* 'herring' is obviously derived from the fact that these fish occur in very large shoals, and the same argument applies to some Germanic words meaning 'spawn' which are quoted in this connection. The respective priority of the meanings of the Latin word should therefore be reversed.

It has been customary to put with Lat. *scateo*, etc. Lith. *skàsti (skantù, skataũ)* 'to jump, spring', the idea being that a meaning 'to rise up' is common to Lat. *scateo* 'to spring up, well up' and Lith. *skàsti* 'to jump'. This connection is impossible when it is seen that the primary meaning of Lat. *scateo* is 'to abound, be abundant', since the meanings are different. There is a good etymological connection for the Lithuanian verb with Gk. (Hesych.) ἐσκατάμιζεν 'jumped'. This is a denominative verb from a **skatamos* formed from a root *skat-* which is identical with that of the Lithuanian verb. To judge from Lithuanian this root is different from the previous one, which has palatal *k (sk̂at-)*, in having a velar *k (skat-)*. The etymological connection of the Greek and Lithuanian words is clear, but they have nothing to do with Lat. *scateo*, etc., and they should form an independent entry in an Indo-European etymological dictionary.

3. *pitta-* n. 'bile'

No satisfactory etymology for this word has so far been proposed. Since it was thought that 'bile' might be so called on account of its colour, as is sometimes the case (Gk. χολή etc.) it has been supposed that there might have been some connection with the adjective *pīta-* 'yellow', but no way has been found to show how these words could be formally connected. A Dravidian explanation which I had attempted (cf. Burrow, *BSOAS* 11. 1948: 345; 12. 1949: 385) based on the same semantics connecting it with Ta. *pacu/pai*, etc. might have accounted for the irregular phonetic correspondence between *pitta-* and *pīta-* which has to be assumed in the case of this as well as of the previous etymology, but it runs into difficulties equally great when examined in detail. This proposed derivation of *pitta-* and *pīta-* was not included in DED 3161.

Any etymology of Skt. *pittá-* must be based on a definition of its structure. It contains the accented suffix *-tá*, which is the suffix of the past participle passive. Consequently we must look for a verbal root in the first part of the word, and this could have been either *pit-* or *pid-*. No such roots are found in Sanskrit, but there is always the possibility that some such root could have existed in the prehistoric period of the language and though obsolete as a verb, could have been retained in this derivative. So we must look to find whether any suitable such root can be found elsewhere in a related language, which would account for this word. Such a root can be found, and it is in fact *pĭ̄d-*.

Greek has the following words showing the root-form *pīd-*: πῖδαξ 'spring, fountain', πιδακόεις 'rich in springs', πιδάω 'to swell out, gush forth' πιδύω 'id.', and πίδυσις 'leaking'. The Greek words have long *-ī-* which contrasts with the short *-i-* in *pid-* which is assumed for the word *pittá-*. This variation will be dealt with below, but for the present we are concerned with the meaning of the root. 'Bile' is secreted in the liver and discharged into the gall-bladder, and and it is the result of this process which is denoted by the word *pittá-*, derived from a root *pid-* having an appropriate meaning.

We may also see a derivative of this root in *piḍaka-* 'boil'. There are two forms of this word used in Sanskrit, *piṭaka-*, and *piḍaka-*, but it can be seen that the more original form is *piḍaka-*, by comparison with the Pali form of the word *piḷakā*, where intervocalic *-ḷ-* corresponds to original *-ḍ-*. The form *piṭaka-* has arisen by hypersanskritisation. The *-ḍ-* of *piḍaka-* is yet another case of spontaneous retroflexion, to which reference has already been made in connection with the word *chaṭā*. The original form was **pidaka-* from this same root *pid-*, and it is so called because it secretes and exudes pus.

The root *pid-* is an extension of a simpler root *pi-*, which is in common use in Indo-Aryan and Iranian: *páyate*, etc. 'to swell, overflow, be exuberant, abound', used primarily of milk in the udder, but also in a more general sense. The short *-i-* of this root is attested by the forms of the perfect participle active: Vedic *pipyúṣī*, Av. *pipyūšī*. The same form of root is also found in Gk. πίτυς 'pine', with the suffix *-tu*. The pine is so called because it exudes resin.

There is also a long-vowel extension of this root producing a root *pyā-* (*ápyāyate*, etc.), having the same meaning as the simpler form of the root. The weak grade of this extended form of the root is *pī-*, as occurring in the past participle *ápīna-* (with an alternative *āpyāna-*). This form with long *-ī-* is also found in Lat. *pītuīta* 'discharge from the nose'; also in Lat. *pīnus* 'pine', and in Skt. *pītu-dāru-* 'a kind of tree (deodar or khadira?)'. It is also this form of the root which has received the extension *-d* in Gk. πῖδαξ, etc.

The root-forms *pī̆-*, *pyā-* and *pī̆d-* and their derivatives form a mutually consistent set with common basic meaning. In particular the words for 'pine' are straightforwardly derived from this root, and there is no reason to suppose that they are not Indo-European in origin (Benveniste, *BSL* 51.1954: 31) or that they are an 'Indo-Mediterranean group'. In the etymological dictionaries the above words have been associated with other word-groups which should be separated from them. Among these are the words for 'fat', Skt. *pī́vas-*, etc. The meaning of this series is quite different inasmuch as fat is a solid deposit and is not discharged. The kind of verbal root that would be suitable for a word meaning 'fat' is illustrated by the word *vapā́*. I have dealt with this word elsewhere (Burrow, *BSOAS* 45. 1982: 188), and shown that the meaning is not 'omentum' as usually given in the dictionaries, but the internal deposit of fat inside an animal and that it is derived from the root *vap-* which means 'to cast down, deposit' particularly where an accumulation results from this action (as in *vapra-*). The word in this sense is therefore not different in derivation from *vapā́* used in the sense of heap in *valmīka-vapā́* 'anthill'. By contrast the root *pi-* and its extensions are quite unsuitable to form the basis of a word meaning 'deposited fat'. The root *pī̆-* of Sanskrit *pī́vas-*, etc. does not, in fact, appear to be a verbal root.

Another word which should be kept separate is *pitú-* 'food'. This has been attached to the above set mainly as a result of a wrong statement of meaning ('Saft, Trank': Pokorny 1959: 793–794). Its only meaning is 'food', and it refers primarily to solid food. The word *pitú-* is to be derived from the Indo-European root *pā-* 'to feed', and it contains the root in its zero grade followed by the incremental vowel *-i-* and the suffix *-tu* (cf. further Burrow 1979: 44).

The adjective *pīta-* 'yellow' is still without certain etymology, but from what has been said it is clear that it can have no connection with *pittá-* 'bile'. H.W. Bailey compares Khotanese *pe* 'colour name, green(?)' (Bailey 1979a: 248), which is possible, but since the meaning is conjectural, somewhat uncertain. Another possible connection would be with Gk. (Hesych.) πίαλυς · παράλευκος.

4. *biḍāla-* 'cat'

Of the two common words for 'cat' in Sanskrit the word *mārjāra-* is clearly derived from the root *mṛj-/mārj-* 'to cleanse, wipe', but the derivation of *biḍāla-* has remained uncertain. According to M. Mayrhofer (1963: 429) it is "nicht sicher erklärt; wohl ein Fremdwort". The normal form of this word which is also the more original form, is *biḍāla-*, corresponding to which there are Pa. *biḷāla-* and Pkt. *biḍāla-*. Pali has also a form *biḷāra-* showing the familiar alternation of *r* with *l*. There are late lexical forms *bilāla-* and *birāla-*. Of these the first *-l-* in *bilāla-* is the *-l-* which in Prakrit corresponds to the *-ḷ-* of Pali, and therefore to the *-ḍ-* of Sanskrit. In *birāla-* there is a metathesis of the consonants of Pali *biḷāra-* and a corresponding Pkt. **bilāra-*. This is the form most commonly represented in the modern languages (Nep. *birālo*, etc., Turner 1966 no. 9237). There is also a hypocoristic *billā, billī* 'cat' of which the latter form has been adopted into Dravidian (DED. no. 3438).

An attempt was made by me to compare this word with the Dravidian words for 'cat' (Ta. *veruku*, etc. DED 4520). There were two faults with this comparison which make it unacceptable. In the first place it assumed that the form *birāla-* was the original form, but it is quite clear that this is a secondary development in Indo-Aryan. In the second place only

the first syllable of the Dravidian word was compared, but in all Dravidian languages without exception this syllable *ver-* is followed by a *-k-*, which is therefore an integral part of the Dravidian word. Consequently any loanword from Dravidian would be expected to show this *-k-*, as has actually happened in Nahali, which has borrowed this Dravidian word in the form *berko/berkū*.

Before going into the question of the etymology of Skt. *biḍāla-* it is necessary to say a few words about the historical phonology of Sanskrit *b*. It has recently become increasingly obvious that Indo-European *w* (*u̯*) which normally appears in Sanskrit as *v*, is alternatively in quite a number of instances represented by *b*. In some cases where this happens there is an alternative form with *v*, as is the case with this word which is also written *viḍāla-*. In other cases only the *b*-forms are attested. Examples of this *b* < *v* are as follows:

baṣkaya- epithet of growing calf < **bakṣaya-* < **vakṣaya-*; cf. (Burrow, *JRAS*, 1969: 114).

bata- 'bad'; Khotanese *bata-* 'small; reverse to good', Zor. P. *wt vat* 'bad', etc., H.W. Bailey (1979a: 95).

barsva- 'gums of the teeth' < **wolswo-* : cf. Gk. *οὖλος* (Studies . . . L.R. Palmer, Innsbruck, 1976: 40).

bali- 'offering; tax': Khot. *gvar-* 'to distribute', Ossetic D *iuarun* 'distribute, divide', etc., Bailey (1979a: 95).

bāla- 'ignorant, foolish; child': cf. OSlav. *volŭ* 'ox', cf. (*BSOAS* 38. 1975: 76).

bāṣpa- (also *vāṣpa-*) 'tears; steam, vapour' < **vāpṣa-*; Lat. *vapor/vapōs*; cf. (*JRAS* 1969: 114).

The Sanskrit word *bala-* n. 'strength, force' is instructive in this respect. H. Grassmann derived it from **vala-*, and compared Lat. *valor*. This was unsatisfactory to the Junggrammatiker, since it went counter to their doctrines that sound rules had no exceptions, and it was clear that the normal representation in Sanskrit of Indo-European *w (u̯)* was *v*. As a result an alternative etymology was proposed, connecting it with Gk. *βελτίων* > Lat. *debilis*, and OSlav. *bolijŭ*. Considering this to be unsatisfactory for various reasons, an alternative derivation from Dravidian was proposed by me (Burrow, *TPS*, 1946: 19) connecting it with Ta. *valam*, etc. (DED 4317). The main objection to this is that a word so central to the Vedic vocabulary is unlikely to have been a loan from a non-Aryan source. Furthermore, the existence of comparative and superlative forms from this root (*bálīyān báliṣṭha-*) is evidence that an Aryan root is involved. These forms would not have been derived from a neuter noun which was a loanword. For these reasons Skt. *bala-* is not being included as a loanword from Dravidian in the revised edition of the Dravidian Etymological Dictionary. After all this H.W. Bailey has returned to the etymology of H. Grassmann, and he now proposes to connect Skt. *bala-* with the Indo-European root *u̯al-* 'to be strong' (Bailey, *BSOAS* 42.). In view of the frequency with which Indo-European *w (u̯)* is now known to be represented by *b*, there is no doubt that he is correct in doing this, and that the earliest etymology of *bala-* is the right one.

We have also to consider the nature and origin of the intervocalic *-ḍ-* in *biḍāla-*. It has already been pointed out that a dental in such a position is frequently converted to a retroflex by a spontaneous process. Furthermore when such a retroflex cannot be explained by combinatory change (as in *nīḍa-*, etc.), then it must be a case of spontaneous change in

words which are originally Indo-Aryan. Such was the case with the words *chaṭā* and *piḍaka-* treated above, and this is the case here. Taking this into account, and also the fact that the initial *b-* is from *v-* (which also occurs), we get back to an original form **vidāla-*. This can be analysed as *vi-dāla-*, of which the first member is the word *vi-* 'bird', and it means the animal which attacks and tears to pieces (small) birds, which is one of the most characteristic habits of the cat. An alternative analysis, taking *vi-* to be the usual verbal prefix *vi* would not be specific enough to provide such a name.

The verbal root involved is *dal-, dalati* 'burst, split' caus. *dalayati* and *dālayati.* This has been considered to be a variant of *dṛ-, dṛṇāti* from Indo-European *der-* (Mayrhofer 1963: 24). In this case the form with *-r-* (Pa. *biḷāra-*) would be the more original. On the other hand there is also evidence for an Indo-European root *del-* with the required meaning (Pokorny 1959: 194), and since the occurrence of *-l-* in Sanskrit usually indicates Indo-European *-l-* this may be the root which is represented in Skt. *dalati, dālayati*, and consequently in *biḍāla-*.

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On Sanskrit *bhunákti* 'aids, serves, protects'

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1. Pāṇini (Aṣṭādhyāyī 1.3.66: *bhujo 'navane*) provides that *bhuj* takes ātmanepada affixes if the verb is used in a meaning other than 'aid, protect' (*anavane*); that is, in the Sanskrit of Pāṇini's time one used middle forms *bhuṅkté* etc. meaning 'enjoy, use, consume', but active *bhunákti* etc. 'aid, serve, protect'. Now, Pāṇini derives *bhuṅkté* and *bhunákti* from a single verb listed in his verb catalog (his dhātupāṭha) but *bhujáti* 'bends' from another verb, for formal reasons: *bhuṅkté, bhunákti* are formed with the same affix, and *bhujáti* has a different affix. Pāṇini does not make any other formal connection between *bhuṅkté* and *bhunákti*. In particular, he does not treat the latter as somehow a causative with respect to the former; on the contrary, only the formal causative *bhoji* (*bhojáyati* etc.) is considered to bear this relation with *bhuṅkté*. This state of affairs reflects a historical development which I shall consider here briefly.

2. In early Vedic, as is well known, *bhuṅkté* etc. are construed regularly with instrumental forms referring to whatever serves to give one enjoyment, benefit or nourishment.[1] The sources of enjoyment can be as general as ones desires (*kāmaiḥ*, e.g. Maitrāyaṇī-saṃhitā [MS] 4.2.1 [21.19]: . . . *sárvaiḥ kā́mair bhuṅkte*) or as vital as life and breath (e.g., MS 3.3.1 [33.1]: . . . *ā́yuṣā ca prāṇéna ca bhuñjáte*). Among the things Vedic poets seek for their benefit is the help of the gods; e.g., Ṛgveda (RV) 8.67.16: *śáśvad dhí vaḥ . . . ā́dityā ūtíbhir vayám/purā́ nūnáṃ bubhujmáhe* 'Ādityas, ever have we enjoyed your help, before and now.' With the help of gods, one has, for instance, cows to subsist on; thus, Indra is asked to return cows that have gone off, RV 10.19.6cd: *púnar na indra gā́ dehi/jīvā́bhir bhunajāmahai* 'Indra, give us back our cows, we would enjoy them alive.' Living cattle were of course used as draught animals, to give milk, and to give more cattle. They could also be eaten; e.g., Kauṣītaki-brāhmaṇa (KB) 11.2.29–32: *tad yathā ha vā asmiṃl loke manuṣyā́ḥ paśū́n aśnanti yathaibir bhuñjate evam amuṣmiṃl loke paśavo manuṣyā́n aśnanti evam ebhir bhuñjate* 'As in this world men eat animals, as they make use of them, so in yonder world animals eat men, make use of them.'[2] As expected, *bhuj* is also used with general terms referring to food one consumes (e.g., *ánnena* in Taittirīya-saṃhitā (TS) 6.2.5.4.: . . . *ánnena bhuñjate*).

3. It has equally been known for a long time (see, e.g., PW V.303) that active *bhunákti* etc. can be construed with accusative forms referring to what is served by another.

3.1. TS 2.6.2.3–4[3] concerns an invitational verse and an offertory verse (*puronuvākyā, yājyā*)[4] which together have seven verse sections, so that they constitute a saptapadā śakvarī, a potent (*śákvarī*) strophe, by whose power (*yásyai vīryéṇa*) certain things are done: One defeats enemies already born and wards off in advance enemies yet to be born, one creates light for oneself in both worlds, an ox helps (*bhunákti*) with its forequarters, a cow with its hindquarters.[5] The power of the śakvarī strophe is apportioned between the puronuvākyā and the yājyā verses. The name of the deity addressed, Agni, occurs in the first part of the former (*agníḥ* in pāda *a*), in the last part of the latter (*agne* in pāda *d*), so that the puronuvākyā and the yājyā are said to have their characteristic marks – the name of the deity – respectively in front and in back. Thus, with the power of the śakvarī strophe that resides in the puronuvākyā one defeats just those enemies that are born beforehand, with the power in the yājyā those enemies that are to be born later; similarly, by the power in these two, one sets down for oneself luster in this world and in the world yonder, respectively, and both worlds are full of light for him who knows thus. Moreover, it is because of this division of power that an ox helps its master with its forequarters, a cow with its rearquarters, and both of these serve him who knows thus: *yá evám véda bhunktá enam etaú.*[6]

3.2. The cow and ox are also said to be of greatest use in a passage from the Śatapatha-brāhmaṇa[7] which gives an explanation of why the Yajamāna is not to partake of these animals when the Adhvaryu has him go to the ritual shed in the course of the Agniṣṭoma. The cow and ox maintain all this that there is. The gods said that, since the cow and ox maintain all there is, they should put in them the vital strength of other animals. They did so. Therefore, the cow and ox most help one: *tásmād dhenúś caívānaḍvāṃś ca bhūyiṣṭhaṃ bhunktaḥ.*[8] If the Yajamāna partook of these creatures, in which the essential strength of all animals was put, then, he would be eating all there is, including the fetus of his wife, if she were pregnant, so that people would say of him that he had killed this fetus, committing a sinful act, thus acquiring a sinful reputation.

3.3. Other brāhmaṇa passages speak of cattle (*paśávaḥ*) serving one. A group of related texts concerns an offering cake that, in the course of the Vaiśvadevaparva rite in the Cāturmāsya, is taken from its kapāla and put in a vessel into which ghee is poured. The cake is equated with the Yajamāna, the ghee with cattle. The Taittirīyabrāhmaṇa[9] says that by pouring ghee over the cake in the vessel, the Adhvaryu makes the Yajamāna rich in cattle. Now, if the Adhvaryu poured only a little ghee, only few cattle would come to the Yajamāna (*úpatiṣṭheran* 'would approach as subjects'), so that they would not really help him (*ábhuñjantaḥ* 'not serving'). On the other hand, if the Adhvaryu pours a lot of ghee, many cattle come to the Yajamāna helping him (*bhuñjántaḥ*). Hence, the Adhvaryu should pour a lot of ghee, but leave the top of the cake exposed: thereby, many cattle come to the Yajamāna helping him. Other texts bring up a possible question about why the cake should be left with its top exposed. One could worry that, if the Adhavaryu completely filled up the pot with the cake in it, thus covering the cake, he would make the Yajamāna lower than the cattle, so that they would not serve him when they came to him. Therefore, one should leave the top of the cake uncovered. This lets the Yajamāna stand above the cattle, which thereby serve him. The texts conclude, however, by saying this is not something to worry about, so that the pot should be filled; for cattle never do not serve their masters: *ná hí paśávo ná bhuñjánti.*[10]

3.4. Of course, cows serve their masters by giving milk and bearing calves, oxen by working as draught animals (see notes 2, 5, 9). Further in Indian imagery, as is well known, cows are also grant-wishing animals (*kāmaduhaḥ, kāmadughāḥ, kāmadhenavaḥ*). Now, the Yajamāna invokes the fire deity, Agni, asking that the bricks piled to make the altar be cows for him in this world and the one beyond, thus helping him in both worlds.[11] As the Maitrāyaṇī-saṃhitā puts it, these bricks are cows that Prajāpati used to milk whatever he desired; the Yajamāna similarly uses them to milk whatever he desires.[12]

3.5. Another well known ritual image is that of the sacrificial fire (*agni*) as transporter (*vahni*) of gods and offerings, and this is linked to the Yajamāna's giving a draught ox as a ritual fee after the ritual fire has been set up. The sacrifice is the yoke-piece or burden that Agni bears as transporter, so that the fire, once it has been set up, aids the Yajamāna (*bhunakty enam agnir āhitaḥ*) by serving as transporter.[13]

A ritual implement too can be said to help in a rite. Thus, the ladle called *darvī* is asked to fly to the gods when it is full of oblations, then to return, bringing with it the fruits of the offerings: It is to bring to the sacrificers nourishment and strength, thus aiding all the sacrifices.[14]

3.6. On a more mundane level, *bhunákti* is used in speaking of an offspring that cares for a parent. The Taittirīya-brāhmaṇa[15] relates a myth that Prajāpati gave rise to creatures through the Vaiśvadeva rite and that these offspring did not procreate. Agni then desired to make the offspring procreate. He caused Prajāpati pain, by reminding him that his creatures did not procreate, and he himself also suffered pain, since he desired but did not have offspring. Thus it is that both he whom an offspring cares for (*yáṃ ca prajā́ bhunákti*) and he whom no offspring cares for both suffer pain.[16]

Those who care for one can also be members of a full household. In the Sāṅkhāyanāraṇyaka (SA) and the Kauṣītaki-brāhmaṇopaniṣad (KBU)[17] a comparison is drawn between how the knowing self is related to its subservient selves – speech, sight, hearing, mind – and how the head of a household is related to members of his household. The selves subservient to the knowing self follow him as members of a household follow their head; and as the head of a household enjoys the use of its members (*yathā śreṣṭhī svair bhuṅkte*) and these members serve the head (*yathā vā svāḥ śreṣṭhinaṃ bhuñjanti*), so does the knowing self make use of the subservient selves (*evam evaiṣa prajñātmaitair ātmabhir bhuṅkte*) and these serve this self (*evam evaita ātmāna enam ātmānaṃ bhuñjanti*).[18]

Just how the subservient selves might be considered to assist the knowing self is brought out in a passage from the Kauṣītaki-brāhmaṇa[19] that concerns invitational and offertory verses which, instead of being recited according to their order in the Ṛgveda, are mixed: RV 4.10.1 is paired with 4.10.3 and RV 4.10.2 is paired with 4.10.4. A parallel is drawn with the breaths, which are said to serve the physical self as though mixed (*vyatiṣiktā iva vā ime prāṇā ātmānaṃ bhuñjantīti*), that is, related to each other, with each one doing its proper job.[20]

4. The relation between the middle *bhuṅkté* etc. and the active *bhunákti* etc. in usages such as those given is fairly obvious and can be shown schematically by the following:

(a) $N^1_{nom.}$ $N^2_{instr.}$ $bhuj_{mid.}$ 'X helps, benefits himself through Y.'

(b) $N^2_{nom.}$ $N^1_{acc.}$ $bhuj_{act.}$ 'Y helps, benefits X.'

This has been recognized not only by modern scholars[21] but also by earlier Indian commentators. For example, commenting on KB 11.2.30, Udaya paraphrases *ebhir bhuñjate* with *ebhih paśubhiḥ . . . ātmānaṃ pālayanti* 'care for themselves through these animals . . .' (see section 2 with note 2). Commenting on part of a verse cited in Yāska's Nirukta,[22] Durga paraphrases *na bhunakti* with *na pālayati* 'does not aid' and explains that this means traditional learning handed down to bad students does not bring these students together with the fruit of this learning.[23]

4.1. There are related sentences with middle and active forms of *vṛdh* which are comparable to examples of constructions (a) and (b). For example, RV 6.37.5b: *índro gīrbhír vardhatām* . . . 'May Indra . . . grow strong through the hymns' has a middle form (3sg. imper. *vardhatām*), and RV 7.99.7c: *várdhantu tvā . . . gíro me* '(Viṣṇu,) let my hymns . . . make you grow strong' has an active form (3pl. imper. *várdhantu*), the first construed with the instrumental *gīrbhīḥ*, referring to hymns through which a god is to grow strong, the second construed with a nominative (*giraḥ*) denoting hymns that make a god grow strong and an accusative (*tvā*) referring to that god. In addition, however, the formal causative *vardhi* can be used as equivalent to active forms of *vṛdh*; e.g., RV 5.11.5cd: *tvā́ṃ giraḥ . . . vardháyanti* . . . '(Agni,) hymns make you grow' has *giraḥ . . . vardháyanti* with the accusative *tvā́m*, just as RV 7.99.7c has *várdhantu . . . giraḥ* with the accusative *tvā*. The causative *vardhi* can also be used in another way, as in RV 1.91.11ab: *sóma gīrbhíṣ ṭvā vayáṃ vardháyāmo* . . . 'Soma, we make you grow strong with hymns.' This involves not merely a god and hymns but also third participants, those who use the hymns to make the gods grow strong. From its earliest attestation, in the Atharvaveda (AV), the causative *bhoji* functions in this system of three participants; e.g., AV 19.50.6c: . . . *etád asmā́n bhojaya* '(Night,) let us enjoy that',[24] Lāṭyāyana-śrauta-sūtra 3.6.8: *sarpirmadhubhyām ṛtvijo bhojayet* . . . 'He should feed butter and honey to the officiants.' In contradistinction to *vardhi*, then, *bhoji* is not used as an equivalent to an active form of the base verb. It is instead linked to the middle *bhuṅkté* etc. separately.[25]

4.2. The pairing of *bhojáyati* etc. with *bhuṅkté* etc. is comparable to the pairing of *tarpáyati* 'makes satisfied' with *tŕpyati* 'is satisfied', from another verb in the semantic sphere of enjoyment. For example, TS 2.5.11.3–4: . . . *tŕpyati prajáyā paśúbhir yá eváṃ véda* 'He who knows thus gains satisfaction with offspring (*prajáyā*) and cattle (*paśúbhiḥ*)', TS 2.5.4.3: . . . *tṛptá evaínām índraḥ prajáyā paśúbhis tarpayati* 'Satisfied, Indra gives him satisfaction with offspring and cattle.' As can be seen, both *tŕpyati* and *tarpáyati* are construed with instrumental forms such as *prajáyā, paśúbhiḥ*.[26] Similarly, *bhuṅkte* and *bhojáyati* can both be construed with instrumentals such as *kā́maiḥ* (see section 2) and *sarpirmadhubhyām* (see section 4.1.).

4.3. On the other hand, from quite early on, *bhoji* could be used with both an accusative denoting someone allowed to enjoy something and another accusative denoting the object enjoyed; for example, in AV 19.506c (see section 4.1), *bhojaya* is construed with *asmā́n* 'us' and *etád* 'that'. In this way, *bhoji* was treated in the same way as causatives from other verbs in the semantic field of enjoyment, namely verbs of consuming, such as *pā* 'drink'. For example, in Kātyāyana-śrauta-sūtra 23.4.24: *mṛtaṃ brāhmaṇān bhojayet* 'He should have some Brāhmaṇas eat the dead animal', *bhojayet* is construed with the accusatives *brāhmaṇā́n* and *mṛtam*, as in RV 3.57.5cd: . . . *ihá víśvāṃ . . . yájatrān ā́ sādaya pāyáyā cā*

mádhūni '(Agni,) have all the worshipful ones sit here and have them drink the sweet drinks', *pāyáya* is construed with *yájatrān* and *mádhūni*. In addition, the base verb *bhuj* (*bhuṅkté* etc.) came to be used, as were other verbs of consuming, with accusatives denoting objects consumed; e.g., Sāṅkhāyana-gṛhya-sūtra 3.1.17; *īpsitam annaṃ tad ahar bhuñjīta* 'That day, (the student) may eat what food he wishes.' In contrast to TS 6.2.5.4, where *bhuñjate* is construed with the instrumental *ánnena* (see section 2), here *bhuñjīta* is construed with the accusative *annam*.[27] Moreover, *bhuj* in the sense of 'enjoy, use' also came to be used in this way. For example, in Raghuvaṃśa 15.1: *kṛtasītāparityāgaḥ sa ratnākaramekhalām/bubhuje pṛthivīpālaḥ pṛthivīm eva kevalām* 'After he had given up Sītā, he (Rāma) as king enjoyed only the earth engirdled by the oceans', *bubhuje* is used with *pṛthivīm*. Rāma now enjoyed only the earth he ruled as king, not also Sītā, whom he had given up. Similarly, in Manu-smṛti 8.150: *yaḥ svāmināmanujñātam ādhiṃ bhuṅkte 'vicakṣaṇaḥ/tenārdhavṛddhir moktavyā tasya bhogasya niṣkṛtiḥ* 'A fool who uses a deposit without the owner's permission is to give up half the interest as compensation for such use', *bhuṅkte* 'uses' is construed with *ādhim*.

4.4. The oldest state of affairs for *bhuj* in Indo-Aryan, such that *bhuṅkte* and *bhunákti* were used in related constructions as shown in (a), (b) of section 4, thus was superseded by a new system, in which *bhuṅkté* and *bhunákti* were no longer so directly related syntactically. The seeds for this change were present in the earliest system. To begin with, the pairing of *bhuṅkté* with *bhunákti* in constructions (a) and (b) was not matched by many similar pairings in any productive system. Even *várdhate* : *várdhati*, moreover was an imperfect match, since the derived causative *vardhi* (*vardháyati*) could be used an an equivalent to *várdhati* but *bhoji* (*bhojáyati*) was not equivalent to *bhunákti* (see action 4.1.). Further, *bhuṅkté* not only remained comparable in its syntactic behavior with other verbs of enjoyment, such as *tŕpyati* (see section 4.2. and note 26), it also came to be used with accusatives denoting objects of enjoyment and consuming (see section 4.3.). The result is the state of affairs described by Pāṇini.[28]

Notes

1 See already the Petersburg lexicon (Böhtlingk – Roth 1855–1875 [PW]) V.301. Note, however, that this and other lexicographic sources give a picture that is not completely faithful to the philological facts. PW V.301 lists first the active *bhunakti*, then the middle *bhuṅkte*, both for the primary meanings given: '(1) *geniessen, Etwas zu geniessen haben*, sowohl *zu Nutzen haben, mit Vortheil besitzen*, als vom *Genuss von Speisen*.' Following suit, Monier-Williams (1899: 759) gives *bhunákti* and *bhuṅkté*, in that order, and glosses, 'to enjoy, use, possess, (esp.) enjoy a meal . . .', although he notes parenthetically that the verb mostly takes middle suffixes in these meanings. More recently, Mayrhofer (1963: 507) lists only the active *bhunákti* as the main entry glossed 'geniesst, benützt, verzehrt/enjoys, uses, consumes', although he goes on to cite middle forms of the verb. See also notes 6, 8, 10, 14, 16, 18.

2 In his commentary on KB 11.2.2.30 (edited by E.R. Sreekrishna Sarma 1976), Udaya (1976: 271) glosses *bhuñjate* with *ātmānaṃ pālayanti* (see section 4) and explains that men care for themselves through these animals, the cattle they have, by having them serve as draught animals, milking them, and using them to bear other cattle: *yathā vā ebhiḥ paśubhiḥ vāhadohaprasavadvārātmānaṃ pālayanti.*

3 . . . *keśínaṃ ha dārbhyáṃ keśí satyakāmir uvāca saptápadāṃ te śákvarīṃ śvó yajñé prayoktáse yásyai vīryéṇa prá jātán bhrātṛvyān nudáte práti janiṣyámāṇān yásyai vīryéṇobháyor lokáyor jyótir dhatté yásyai vīryéṇa pūrvārdhénānadvān bhunákti jaghanārdhéna dhenúr íti/purástāllakṣmā puronuvākyā*

bhavati jātắn evá bhrắtṛvyān prá ṇudate/upáriṣṭāllakṣmā yājyà janiṣyámānān evá práti nudate/purástāllakṣmā puronuvākyā bhavati asmínn evá loké jyótir dhatte/upáriṣṭāllakṣmā yājyà amúṣminn evá loké jyótir dhatte/jyótiṣmantāv asmā imaú lokaú bhavato yá evắṃ véda/purástāllakṣmā puronuvākyà bhavati tásmāt pūrvārdhénānaḍvắn bhunakti/upáriṣṭāllakṣmā yājyā tásmāj jaghanārdhéna dhenúḥ/yá evắṃ véda bhunktá enam etaú . . .

[4] Respectively *agnír mūrdhắ diváḥ kakút/pátiḥ pṛthivyắ ayám/apắṃ rétāṃsi jinvati* and *bhúvo yajñásya rétasaś ca netắ yátrā niyúdbhis sácase śivābhiḥ/divi mūrdhắnaṃ dadhiṣe suvarṣắṃ jihvắm agne cakṛṣe havyavắham*, which originally occur as RV 8.44.16, 10.8.6 and are repeated in Taittirīya-brāhmaṇa (TB) 3.5.7.1 and elsewhere.

[5] That is, an ox serves its master by pulling the plow or cart with its shoulders, a cow by giving milk with its udders. Sāyaṇa is, I think, perfectly right when, in his comments on TS 2.2.6.3 (Ānandāśrama Sanskrit Series 42.2 (Pune 1966:1389)) he says *pūrvārdhéna* refers to the front part of an ox, its shoulders, *jaghanārdhéna* to the rear part of a cow, its udders, and when he glosses *bhunakti* with *pālayati* 'helps': . . . *tadīyena vīryeṇa balivardo lāṅgalaśakaṭavāhinā skandharūpeṇa pūrvabhāgena svāminaṃ pālayati/dhenuś ca kṣīrapradenodhasāparabhāgena svāminaṃ pālayati.* See also note 6.

[6] A. Berriedale Keith (1914: 207) translates *bhunákti* 'feeds' and *bhuṅktắḥ* 'enjoy': '. . . by the first half of whose strength the ox feeds, by the second half the cow . . . therefore the ox feeds with its first half . . . therefore the cow feeds with the second half. Him who knows thus these two enjoy.' This makes little sense in the context of the passage. In addition, *yásyai vīryéṇa pūrvārdhéna . . . jaghanārdhéna* cannot justifiably be translated 'by the first half . . . by the second half of whose strength.'

[7] SB 3.1.2.21: *áthainaṃ śắlāṃ prápadayati/sá dhenvaí cānaḍúhaś cá nāśnīyāt/dhenvanaḍúhau vā́ idaṃ sárvaṃ bibhṛtaḥ/té devắ abruvan dhenvanaḍúhau vā́ idaṃ sárvaṃ bibhṛtaḥ hánta yád anyéṣāṃ váyasāṃ vīryaṃ tád dhenvanaḍuháyor dádhāmēti/sa yád anyéṣām váyasāṃ vīryam ắsīt tád dhenvanaḍuháyor adadhuḥ tásmād dhenúś caívānaḍvắṃś ca bhūyiṣṭhaṃ bhuṅktaḥ tád dhaitat sarvắśyam iva yó dhenvanaḍuháyor aśnīyāt/ántagatir iva taṃ hắdbhutam abhíjanitaḥ jāyắyai gárbham níravadhīd iti pāpám akad íti pāpī́ kīrthiḥ/tásmād dhenvanaḍuháyor nāśnīyāt.*

[8] It is neither contextually nor formally justifiable to translate *bhuṅktaḥ* 'eat' (Eggeling XXVI.11: '. . . and therefore the cow and ox eat most.') or 'fressen' (PW V.302: 'fressen am meisten').

[9] TB 1.6.3.4–5: . . . *yájamāno vā́ ékakapālaḥ paśáva ắjyam/yád ékakapāla ắjyam ānáyati yájamānam evá paśúbhiḥ sámardhayati/yád álpam ānáyed álpā enaṃ paśávo'bhuñjanta úpatiṣṭheran/yád bahv ānáyed baháva enaṃ paśávo bhuñjánta úpatiṣṭheran/bahv ānīyāvíhprṣṭhaṃ kuryāt/baháva evaínaṃ paśávo bhuñjánta úpatiṣṭhante.* In his comments on TS 1.8.2. (Vaidika Saṃśodhana Maṇḍala edition 2:319 (Pune 1972: 319)) Sāyaṇa cites this passage, glosses *bhuñjántaḥ* with *pālayantaḥ* 'helping', and specifies that the cattle help by giving milk and similar acts (*bhuñjantaḥ kṣīradānādinā pālayantaḥ*).

[10] MS 1.10.7 (147.2–6): *yájamāno vā́ ékakapālaḥ paśávo ghṛtám/tád abhipū́ryaḥ/paśúbhir evaínaṃ sámardhayati/yád abhipū́rayed ádharam enaṃ paśúbhyaḥ kuryāt/ábhuñjanta enaṃ paśávā úpatiṣṭheyuḥ/āvíḥprṣṭhaḥ kāryàḥ/paśúbhya evaínam úttaram akaḥ/bhuñjánta enaṃ paśávā úpatiṣṭhante/tán ná sū́rkṣyam/abhipū́rya evá/ná hí paśávo ná bhuñjánti.* Similarly, Kāṭhaka-saṃhitā (KS) 36.1 (69.6–10). From such passages it is clear that *bhuñjantaḥ* of Tāṇḍya-mahā-brāhmaṇa 25.1.13: *tasmād arvāñco bhuñjantaḥ paśava upatiṣṭhante* cannot justifiably be said to mean 'eating', as PW V.302 would have it (line 6); the cattle approach with heads lowered (*arvāñcaḥ*), serving their masters by giving milk and such, as Sāyaṇa correctly says (. . . *bhuñjantaḥ payaādinā́ pālayanto gavādipaśavaḥ arvāñco'dhomukhāḥ sāyaṃ svāmigṛham upatiṣṭhante*).

[11] E.g., Vājasaneyi-saṃhitā 17.2: *imā́ me agna íṣṭakā dhenávaḥ santu . . . etā́ me agna íṣṭakā santv amútrāmúṣmiṃl loké*; ŚB 9.1.2.17: . . . *tathó hainam etā́ ubháyor lokáyor bhuñjanti asmíṃś cāmúṣmiṃś ca*; Sāyaṇa on the latter: *enaṃ yajamānaṃ bhuñjanti pālayanti . . .*

[12] MS 3.3.4 (36.3–4): *dhenávo vaí nā́maitā́ íṣṭakāḥ/etā́bhir vaí prajā́patir yád yad ákāmayata tát tad áduha/yád yad evaítā́bhir yájamānaḥ kāmayate tát tad duhé.*

[13] KS 8.5 (89.5), Kapiṣṭhala-kaṭha-saṃhitā 7.1 (84.5–7): *vahī́ deyaḥ/vahino vai paśavo bhuñjanti/yajño 'gner vahaḥ/bhunakty enam agnir āhitaḥ.* The sentence *bhunakty enam agnir āhitaḥ* is given in the Kāśikā-vṛtti to Pāṇini 1.3.66 (see section 1) as an example of *bhunakti* meaning 'aid, protect'. Note that I have adopted the reading *vahaḥ* (. . . *vaho bhunakty* . . . in a saṃhitā reading). Both von Schroeder in his edition of the Kāṭhaka (1900: 89 with note 2) and Raghu Vira in his edition of the

Kapiṣṭhala-kaṭha-saṃhitā (1968: 84 with note 4) adopt *yajño'gner vahī*, although the former mentions that *vahoḥ* occurs in his manuscript D and the latter mentions that *vaho* is the reading of his only manuscript. I have adopted *vahaḥ (vaho)* both because this has manuscript support and because, at least to me, it is the reading that makes sense. As is known, *vaha* denotes an ox's shoulders (cf. Amarakośa 2.9.63: *skandhapradeśas tu vahaḥ*), on which the animal carries the yoke (cf. Pāṇini 3.3.119). AV 4.11.8ab: *mádhyam etád anaḍúho yátraiṣa váha áhitaḥ* 'This is the middle of the ox, where this *vaha* has been put' refers to a *vaha* put on the middle of an ox, that is, on the shoulder region. Sāyaṇa interprets *váhaḥ* as meaning 'load' (*bhāraḥ*), and others have taken it to designate the piece of the yoke that goes on the shoulders (Wackernagel-Debrunner 1954: 68: 'Schulterstück des Jochs'). In either case, *váha* refers to something an ox carries. And Agni the transporter (*vahni)* is equated with a draught ox in a passage related to the two Yajurveda texts under discussion. TB 1.1.6.10–11 says the Yajamāna should give the Adhvaryu an ox (*anaḍváham adhvaryáve*), then equates both the ox and the Adhvaryu with the fire as transport (*váhnir vā́ anaḍvā́n/váhnir adhvaryúḥ*): The ox and Agni are both transporters, the first for the cart, the second for ritual substances (Sāyaṇa: *anaḍvān śakaṭaṃ vahati/agniś ca dravyaṃ vahati*); the Adhvaryu too is a *vahni*, in that he "carries out" rites (Sāyaṇa: *adhvaryur apy anuṣṭhānaṃ vahati*). Through the ox that is a draught animal the Yajamāna gets a transport for the ritual (*váhninaivá vahni yajñasyāvarunddhe*). It is understandable that a Brāhmaṇa text speaks of the sacrifice (*yajñaḥ*) as the *vaha* (*vahaḥ*) of Agni, but it is not immediately understandable that such a text should speak of the sacrifice as the draught animal (*vahī*) of Agni. Hence my choice. To be sure, one could adopt the reading *yajño'gner vahī* and take this to mean 'The sacrifice has a bullock's shoulder in respect of Agni', that is, it has Agni's shoulders to bear it, but this appears tortured, especially since *vahin* regularly denotes a draught animal, not just anything that has an ox's shoulder.

[14] AV 3.10.7c-f: *pūrṇā́ darve párā pata súpūrṇā púnar ā́ pata/sárvān yajñā́n tsaṃbhuñjatīṣam ū́rjaṃ na ā́ bhara.* Whitney's translation (1905: I.101) 'jointly enjoying' certainly lacks any support in the context of a single ladle.

[15] TB 1.6.2.1.: *vaiśvadevéna prajā́patiḥ prajā́ asṛjata/tā́ḥ sṛṣṭā́ na prā́jāyanta/sò'gnír akāmayata ahám imā́ḥ prájanayeyam íti/sá prajāpataye śócam adadhāt/sò'śocat prajā́m icchámānaḥ/tásmād yáṃ ca prajā́ bhunákti yáṃ ca ná tā́v ubhaú śocataḥ.*

[16] While explaining this passage, in his commentary on TS 1.8.2 (Vaidika Saṃśodhana Maṇḍala edition, II.317), Sāyaṇa again paraphrases *bhunákti* with *pālayati.* In RV 8.1.6ab: *vásyāṃ indrāsi me pitúr utá bhrā́tur ábhuñjataḥ* the poet says to Indra, 'You are worth more to me than my father or a brother that does not help.' The allusion is most probably to a brother who does not contribute properly to the maintenance of the joint household; compare the reference to a wealthy man who does not help others (RV 1.120.12b: *ábhuñjataś ca revátaḥ*).

[17] ŚA 6.20, KBU 4.20: *tam etam ātmānam eta ātmāno'nvavasyanti yathā śreṣṭhinaṃ svāḥ/tad yathā śreṣṭhī svair bhuṅkte yathā vā svāḥ śreṣṭhinaṃ bhuñjanti evam evaiṣa prajñātmaitair ātmabhir bhuṅkte evam evaita ātmāna etam ātmānaṃ bhuñjanti.*

[18] Śaṅkarānanda (1968: 124) has the reading *bhuñjate* and explains the simile as follows: The members of the household eat the head of the household by using his substance (*dravyadvārā*; cf. English 'eat out of house and home'). Cowell adopts the reading *bhuñjanti* but translates (1968: 171) 'and as the household feed on the householder', which is not acceptable; better, Renou (1948: 71), 'ou comme ses (hommes) servent le chef.'

[19] KG 1.5.4–5: *vyatiṣiktā bhavanti/vyatiṣiktā iva vā ime prāṇā ātmānaṃ bhuñjantīti.*

[20] In his commentary, Udaya (1976: 25) first explains that *iti* (. . . *bhuñjantīti*) is used here in the sense of 'because' (*iti śabdo hetvarthaḥ*), then equates the breaths spoken of with sight etc.: *yasmād ime paridṛśyamānāś cakṣurādayaḥ prāṇāḥ susambaddhā eva santa ātmānaṃ śarīraṃ bhuñjanti pālayanti . . . pālanaṃ nāma svocitatattatpravṛttihetutvam/sambandhaś cānyonyaṃ sāhāyyācaraṇam/na hi cakṣurādīnām anyonyavārtānabhijñatve tannimittā śarīrapravṛttir upapadyate.* As can be seen, Udaya once more glosses *bhuñjanti* with *pālayanti.* He also explains that the act of taking care of, serving (*pālanam*) consists in being the cause for the body to enter into activity as appropriate to each sense and sense organ. It is not possible that the body engages in any activity of perceiving conditioned by these unless they have separate spheres recognized as such; and their being related consists in their helping each other. It is not necessary to take up an alternative interpretation which Udaya gives.

[21] See recently K. Hoffmann (1967: 96 with note 196). I do not discuss here the optative *bhujema*, used with *mā́*, and the associated first singular form *bhojam*, both used in the Ṛgveda, since at the moment I do not have anything definite to contribute to the problems associated with these; they are treated by Hoffmann (1967: 95–96).

[22] *adhyāpitā ye gurum nādriyante viprā vācā manasā karmaṇā vā/yathaiva te na guror bhojanīyās tathaiva tān na bhunakti śrutaṃ tat* 'As Brāhmaṇas who, when they have received traditional instruction, do not respect their teacher in word, thought, or act are not worthy of having the teacher give them food, so does the learning they have received not serve them.' The verse is cited in Nirukta 2.4.

[23] Durga (Ānandāśrama Sanskrit Series 88.1: 142 (Pune 1921)): *na pālayati na bhunakti/śrutaphalena na saṃyunaktīty arthaḥ.*

[24] The rest of the text involves difficulties not directly pertinent to my discussion.

[25] As *várdhayati* is later connected only with *várdhate*, since the factitive *várdhati* is eliminated. But *bhunákti* semantically distinct from *bhojáyati* is not eliminated.

[26] It does not matter to my discussion that *tṛ́pyati* in the Ṛgveda is construed with a genitive, not an instrumental (cf. Delbrück 1888: 133).

[27] See PW V.301. Note that the use of either an instrumental or an accusative form signifying an object consumed in constructions with *bhuj* persisted for a considerable time. Both constructions are used by Patañjali in his Mahābhāṣya (Pune 1962–1972; e.g., Bhāṣya II.163.2–3 (on Aṣṭādhyāyī 3.3.193): *abhokṣyata bhavān māṃsena yadi matsamīpa āsiṣyata* 'You would eat meat if you were to sit near me', I.326.20 (on 1.4.24): . . . *odanaṃ bhuṅkte* '. . . is eating rice', the first with the instrumental *māṃsena*, the second with the accusative *odanam*.

[28] See section 1. Within the linguistic system in question, moreover, one cannot link *bhuṅkté* and *bhunákti* by the semantic criterion which generally determines whether a verb that partakes in this opposition has middle or active inflection, namely whether or not the results of the act signified by the base is intended for the agent of that act (Pāṇini 1.3.72: *svaritañitaḥ kartrabhiprāye kriyāphale*, 1.3.78: *śeṣāt kartari parasmaipadam*). The best known example is *yájate*, said of a Yajamāna, one who serves as patron of ritual acts and for whom the fruits of these acts are intended, opposed to *yájati*, used of an officiant who performs such acts for the benefit of the Yajamāna. *bhuṅkté* 'enjoys, uses, consumes' and *bhunákti* 'aids, serves, protects' are not related in this manner, and Pāṇini treats the verbs *yaj, bhuj* accordingly: In his dhātupāṭha, *yaj* is given with a svarita vowel as a marker (*yajÀ*), so that ātmanepada and parasmaipada affixes follow this under the semantic conditions noted, but *bhuj* is listed with a high-pitched vowel marker (*bhujÁ*); Aṣṭādhyāyī 1.3.66 then lets this base take ātmanepada affixes when it is used in particular meanings.

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Who did discover the law of the palatals?

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During the 1870s it suddenly occurred to people that two problems of Indo-European history were each the solution to the other. The first was a Proto-Indo-European question: had the 'Ursprache' a basic system of five vowels (as Greek or Latin – or Gothic, once its spelling was understood – and even Celtic seemed to suggest)? Or was it of three, with only /a/ as a non-high segment (as Indo-Iranian offered, and in a quite 'natural' way, in view of the Semitic system)? The other nuisance was inside Sanskrit, which showed palatal stops as reflexes of inherited dorsals where a velar articulation was expected. Scholars were inured to the complexities of the dorsal series, where the skewed pairings of segment-types in the Indo-European languages argued for either three types in Proto-Indo-European, or else two there with collapsing of these (plus phonemicizing of variants of one of them) in one group of languages: on which the battle still rages. But Sanskrit had a secondary palatalization (with a different actual result (*c* not *ś*) in the voiceless case). There is awkward distribution too: forms like *vacas* and *pacanti* seem to palatalize /-ko-/, against the testimony of *cakāra* with /keko-/. The solution, once stated, was unassailable: Sanskrit also would argue for a mid-front /e/ in PIE (and so for /o/ also), were it only granted a prehistoric /e/ of its own; and if it were, the condition for its *-ca-* effect was honoured, and *pacanti* etc. could be seen for the analogical products which they are. (Paradigmatic analogy is a splendid tool for explaining exceptions in a known output, but it is awfully stifling when the rule is not known and the exceptions cannot be seen. Again, Sanskrit was the oldest witness, and so indeed until Hrozný unveiled Hittite in 1917; but this solution showed it to contain derivative explicanda. Hence the delay and the sadness of some – Curtius, for example; cf. Jespersen 1922: 91 – at the demoting of this prime witness.) So at last emerged 'the law of the palatals', which might as suitably have been titled 'the law of the Indo-European vowels'.

Still, as we have learnt from Hoenigswald in so salutary a fashion, an unconditioned merger ('the central type of sound change') is a handier diachronic affair than a complicated process of splitting. So the thesis that /e, a, o/ became /a/ in Indo-Iranian should always have been the horse to back. Perhaps palatalizing before /i/ in Indic would have commended it; but *apaciti-* (Ved.) and *cid* (but *kim*, from MIA) practically exhaust the cited evidence. In fact, the shifts *are* inside Indic and are: (e.g. **(–)$k^{w}e$(–)>*ke>ce>ca*, with paradigm-levelling at the last or next to last stage. But who said all that, or conceived it, first? Jespersen (1922: 90 fn. 1) thought it futile to enquire; and two of the possible claimants themselves deplored the attempt. But investigation is fair in an area where information has been leaked piecemeal and interpreted loosely. At least one should gauge what credit is due, or not, to various contenders and what testimonia might still be rummaged for.

Some of the Indo-Europeanists of the period can be readily eliminated from contention for the 'Prioritätsrecht'. Two were involved simply as triggers. In 1870 Ascoli, having led the way among those who (against Fick) reconstructed three basic dorsal stops in Proto-Indo-European, opted for even a fourth series to solve the Sanskrit riddle. This seemed to touch a nerve; it was seen to flout economy and chronology and never won hearts as the proposed trio of original dorsals still does. Pedersen in 1924 (cf. 1962: 281) spoke roundly of Ascoli's 'mistake'. Some explanation was urgently needed. Then in 1874 Amelung repeated the suggestion (which was already in Benfey 1837) that Proto-Indo-European and Greek /e/ had collapsed with /a/ in Sanskrit; but he showed more precision and a very relevant concentration on /e/ alone. Amelung does not see the crucial link with the palatalized dorsals, nor credits anyone else with it: so 1874 is the terminus a quo for the discovery. And by 1879, with the treatments of Collitz and Hübschmann published in that year, the thing was quite codified. We have our limiting quinquennium.

To those set aside (Benfey, Ascoli, Amelung) we can add others who contributed nothing, or only on the array of basic velar consonants (Fick, Bezzenberger, Bugge). Some ten candidates remain, and even of these five can, with a trifle more trouble, be winnowed out. From the 'document in the case' #3 – of which more below – it emerges that news of the law came quite fresh to Leipzig in 1876 and was so received by Osthoff and others, four of whom are named by him. Three were Brugmann, Hübschmann and Leskien. None of these is ever put forward as the discoverer; and only Hübschmann and Osthoff discussed it in print at the time and both assigned credit elsewhere (in fact to Verner). The other named recipient of the news was Saussure.

Now Saussure is said by Szemerényi (1964: 4 fn. 6) to have been 'very near to discovering the law' (but cf. Szemerényi 1967: 68 fn. 1); and Streitberg (1913; cf. 1915: 205f.) had written of him as among its discoverers (as also does Koerner as recently as 1978: 199). Indeed, Saussure makes a claim for himself: "il me vint l'idée . . ." (1879: 369). Saussure was undoubtedly precocious (cf. Pedersen 1962: 284 fn. 1; he was "der frühreife Schweizer" for Brugmann 1909: 219); still he was only 18 at the critical time in Leipzig. Had he revealed then that the solution was already in his mind (in October 1876) – and if he did not then his claim fails – he must have provoked comment or challenge from those quick wits. There was his remark to Streitberg in 1903 (see the latter's 1915 notice) that he had *not* heard Verner's news in Leipzig; and on 21 July 1877 he read a paper (which was published in 1879) which antedates the *publishing* of the law by Collitz and Osthoff. But those scholars must have been working on the material months before. Saussure's famous *Mémoire* (of December 1878) -- the great 'prelaryngealist' paper – combines perspicacious morphology, as on the Proto-Indo-European nasal infix verbs, with really obscure, if 'anregend', phonology. As to the 1879 publication on the palatals business (given as a paper in 1877), Saussure admits (1879: 1 fn. 1) that his essay is overcondensed and short on data and coherence; only two pages are ad rem. It is suggestive that he never repaired these faults as promised. He wants a single non-high Proto-Indo-European vowel /a/ (1879: 362) but with various "puissances" (1879: 363), with no phonological motivation. So we are less far forward than was even Amelung in 1874. Four Brugmann-like variants are offered: A, A_2; a, a_2. The first two respectively relate to some cases of $\bar{\imath}$ and $\bar{u}$ in Sanskrit and some 'European' cases of o (A), and to Brugmann's law output (A_2). The second pair answer in turn to 'European' /e/ and to most cases of /o/. True, *cakāra* is explained as from *$*k_2ak_2A_2ra$*, so that the velars are affected by the following vowel; but when *arca-* vies

with *arka-* (etc.) we hear that *arcati* has /*-ka-*/ but *arcāmi* has /*-ka*$_2$-/ (1879: 369). This should mean that they share frontness, and that is untrue; analogy is not mentioned and even the frontness feature is left inexplicit. Even Streitberg (1915: 206) concedes that here is error and failure. In the second syllable Greek *épos* is assigned a_2 but *lúkos A*, which is ad hoc. If these are insights, they are provokingly dark; if this is pioneering, it is rather late. (And on the derivative nature of much of Saussure's work, cf. Collinge 1979: 209; Koerner 1978: 149.) So the entire Leipzig group may stand aside.

We thus retain a short-list of five:

Collitz, Hermann (1855–1935)
Schmidt, Johannes (1843–1901)
Tegnér, Esaias Henrik Vilhelm (1843–1928)
Thomsen, Vilhelm Ludvig Peter (1842–1927) and
Verner, Karl Adolf (1846–1896);

– and we have five crucial 'documents in the case':

#1 = Schmidt 1881; #2 = Collitz 1886 (& 1887); #3 = Osthoff 1886; #4 = Verner 1886; #5 = Thomsen 1920.

The law was first published in 1878. In Collitz 1878, which appeared in May, it is really only presaged on the last page (305); and this is made good the next year. Osthoff 1878 sets out the law, and gives credit to Verner for having had the notion '1½ to 2 years' before; but Collitz is heavily ironic (1879: 207 fn. 1) over Osthoff's failure to find a copy of his own earlier article; and (if Osthoff must prize thought over print) Collitz claims to have ventilated it all verbally in the summer of 1876 with Fick, Bezzenberger and Benfey. Of this there is corroboration (in *BB* 12 (1887): 248). But even so, it is a shame the date Collitz quoted was not a year earlier, as we shall see.

'Document #1' was submitted to *KZ* in June 1879, and Schmidt says he was writing it in the spring of 1878. It links Indo-Iranian palatalization to following front vowels. But in *KZ* for 1877 (vol. 23) Schmidt had an article siding trenchantly with those who viewed a Proto-Indo-European /e/ – or even a common European vocalism – with scepticism. Such a volte face within a year is remarkable, if unprompted. Even more remarkable is his writing (1881: 63) that he could well have claimed the inventor's right and is constrained from doing so only by not having the time or inclination to deploy the necessary Avestan and Vedic evidence (and by one or two troublesome exceptions); but he has been including the law 'since May 1877' in his lectures – a phrase to bear in mind. He does not want an unfruitful combat over the title of discoverer, and anyhow Thomsen of Copenhagen 'probably had the idea earlier than all of us'. That sounds as if counterclaims alone were restraining him. He insists on the second-hand status of Hübschmann's and Osthoff's knowledge in the affair, and declares that he had *written* his version before the publications of Collitz or Hübschmann or Saussure. But the phrase 'May 1877' is a curiosity. He had, according to 'document #3', been told of the law in January 1877; so the remark is off the point and shows rather that he took a longish time to assimilate the implications. (Four months *is* a longish time in a matter where Collitz censures Osthoff for being two months late.) Finally, nobody but Schmidt supports Schmidt's claim (implied even in 1884).

'Document #2' was written by Collitz in 1885. As he was in many ways a Fick man and Schmidt was a pupil of Schleicher it is intriguing to find here some German solidarity – but it is for outflanking the Leipzig school that Schmidt wins Collitz's praise. The article

concedes, however, that Thomsen, Verner and Saussure may be independent discoverers of the law. We have considered Saussure; the others remain possible. Tegnér is also mentioned as a co-inventor (the news comes from a letter from Thomsen, which speaks of the law as 'lying in the air' – a recurrent phrase in this affair). So Collitz seems to forgo his claim (and, by publication priority, *a fortiori* to disclaim the honour for Schmidt); his conversations of 1876 fade into nothingness. Yet Prokosch, always an encomiast of Collitz, gives him credit for this law (1939: 93) and is followed by Porzig (1954: 27); more arresting is that Allen (1973: 123) should think Collitz 'an independent discoverer of the law'. This judgement may have some colour.

Then came 'document #3', by Osthoff, who writes in fury over the stealing of Leipzig thunder by Collitz and Schmidt. The nub is that Osthoff and others learnt the news of the law from Verner "at the beginning of the winter semester of 1876" (in fact, October 1876) – and the major names are given of those who received this bolt from the blue. Later, writes Osthoff, in January 1877 he visited Schmidt in Berlin and passed on the news, which Schmidt seemed to recognize as revolutionary and destructive of many existing explanations of his own as to Indo-European matters; he became pensive ("nachdenklich"). But later he referred to the disclosure (181) as if it had not come via Osthoff; and even in 1884 ostentatiously resigned his claim so as virtually to leave a toe in the door. For Osthoff the law is Verner's, and is twice so characterized; resisting the temptation to organize a colloquium on the subject, Osthoff set it out in print in 1878, with Verner duly honoured.

Verner himself produced 'document #4' in November 1886. It appeared in a journal, and in a year, wherein Collitz and Schmidt are also named as the inventors. Rooth's reprint of the text (1974: 17–20) is very helpful, as was Szemerényi's reminder of Osthoff's statement (1967: 68 fn. 1). Verner starts to the effect that for eleven years he has given up such claims to the inventorship as he once had. Well, then he *had* them. At the end he lyrically exclaims that the solution had been an over-ripe fruit on the tree of philology and after its fall the only credit is to have shaken the tree hardest. So he agrees with Thomsen that the personal question is neither here nor there, and an orphanlike title for the dubious child is best; enquiry into paternity, as the Code Napoléon said, is forbidden.

Yet things said in the centre of the document are rather curious. Verner says he discussed the matter with Thomsen in Copenhagen in the summer of 1875 (when, one must remember, Collitz was only 19). Now that spring and early summer Verner was at Aarhus recuperating, and working on those two famous papers which appear back-to-back in *KZ* 23 (dated 1877, but the early papers are said to have appeared in 1876). Of these the first sets out Verner's own famous law, which involves him in handling unfixed word accent in Indo-European. The second treats of the implications of his law for the theory of ablaut. That, of course, is the natural outcome of interest in Proto-Indo-European accent positioning, and should lead him on to feeling a need for a mid-front and a mid-back vowel in Indo-European (if ablaut was then taking the shape in his mind which it later took in theory). When, however, he found that Thomsen had independently come to the same result by another path, he had no doubt that Thomsen deserved recognition as the inventor (and hoped and expected that Thomsen would write a paper on the subject). This is less straightforward than it appears. Thomsen's paper is printed in his collected works (the second volume of which came out in 1920). It certainly states the law; it does so, however, on the basis of control by contextual variants of a single low vowel; and although it is considerably tidier it is not substantially different from the approach of Saussure (or of Brugmann

on Indo-European vocalism). There is absolutely nothing of the dynamic inter-theoretical linkage of which we might well believe that Verner was capable. True, one is merely surmising that, having gone from his own law to a discussion of ablaut, Verner had mentally pursued ablaut theory to the point of revising the Proto-Indo-European vowel inventory. It is possible that he had *not*, and that it only occurred to him at the moment when he heard what Thomsen, from another point of view, had to say. But what if Thomsen simply established to Verner's satisfaction that he (Thomsen) had been thinking about all this at an earlier time? Then, for Thomsen to substantiate *that*, he would have to claim to have been so thinking early in 1875. Yet we have seen that the terminus a quo of this whole business is Amelung's paper of 1874. So, all that Thomsen could effectively say is that he thought of the law in 'late 1874' or 'early 1875'. And that is exactly what in his own statement he does say.

To continue, however, with Verner. He then reveals that he wrote a letter in August 1875 to the editors of *KZ* in which he reported both the essentials of the law and that Thomsen already had some right to it (had, in fact, spoken of it to some audience or other) and was about to write an article on it. Then, he writes, when he felt fitter and went to work in Halle he began to visit Leipzig; and one evening in October 1876 was involved in an animated philological discussion in a café. His contribution to the lively proceedings was a brief outline of the law – including a mention of Thomsen's name. He was upset when he came to think of what he had done; he realised that the thought once spoken was no longer a secret: 'one cannot take out patents in philology', he sadly observes. He does not know whether to blame the company for shrewdly seizing on to the idea at once, and beginning its exploitation, or to blame himself for having destroyed Thomsen's right of priority, as Thomsen had not yet published. He consoles himself with the thought that at least he had written the letter to *KZ* and so protected Thomsen. He allows that Tegnér, too, in Lund had at least begun to write on the subject but was inhibited by a flood of solutions from various sides. This seems to refer to some time before October 1876; but there are problems about taking that as Tegnér's moment, to which we shall come.

Now comes 'document #5'. Between 1919 and 1922 Thomsen published his collected works, and in the second volume (dated to 1920) he finally printed his article on the law. 'Document #5' is its preface. In 1877 he had offered it to *KZ*, the first half being complete and ready for publication and the second in little more than draft form; and this is how it is given in 1920. It did not appear in *KZ*. On its submission Thomsen had a letter from Ernst Kuhn, one of the editors, warning him off, because the solution was 'lying in the air' (or, we might say, 'floating over Europe') – a phrase which clearly pierced Thomsen. Schmidt and Hübschmann were said to be working on papers to the same effect. The latter's was published in 1879 and, as promised, mentioned Verner; Schmidt's article is presumably our 'document #1' (despite the author's dating of its writing to 1878) – which is ironic, as Thomsen's could easily have been given to the world before it. But there are mysteries here. How does it come about that when Ernst Kuhn receives an article on the law from Thomsen in 1877 – admittedly about a year behindhand, because Thomsen visited Oxford by invitation in 1876 and spent a year preparing his lectures in another field and subsequently gathering them into a book – Kuhn responds as he does? Has he not been told already in 1875, by Verner, to expect such an article to be offered by Thomsen, as the law's discoverer? Was the letter really mislaid, or just ignored? Why did not somebody at *KZ* write to Schmidt and Hübschmann and tell *them* to hold back until it was

established whether Thomsen had quit the field or not? E. Kuhn offers an 'explanation' (1886: col. 1840) which shifts the blame without answering our questions: that the letter should fail to pass between the Kuhns over several years (and yet not enough years) is eyebrow-raising. (The editors of *KZ* in 1877 were, by the way, the two Kuhns, Leskien and Schmidt.) On the face of it, Thomsen was not well served; his claim to the discovery is not shaken.

No, the disappointing thing is his *path* to the discovery, which is so much less interesting than the exploitations by the Germans (who linked it to ablaut theory), and than what Verner might have made of it. And there are other oddities about Thomsen's statement. He speaks of agreeing with Verner (referring to 'document #4') in leaving unedifying disputes about right of priority to the German scholars. He also reports meeting Verner in the summer of 1875; but in the many detailed letters which Verner wrote to him about that time, he says, the law was not mentioned at all; and in the conversation in the summer of 1875, when they *did* discuss it, Verner never once suggested that he himself had made the discovery. This would seem to make Thomsen's case very strong indeed. Yet there is the rather elastic way in which he declares that he had thought of it all 'towards the end of 1874 or at the beginning of 1875'; although one might well not manage to place an idea to a month or two, and particularly if it comes out of one's lectures in a given academic session. More worrying is the fact that Thomsen goes on to state that his results were immediately afterwards put forward in lectures: "mine resultater har jeg straks derefter fremstillet i forelaesninger" – a sentence which echoes Schmidt's disingenuous remark of 1881.

Thomsen also gives (in 1920; not in 1902) credit to his friend Tegnér who 'at the same time as myself had come to the same conclusion'. Tegnér's part is hard to assess. He spent most of his career as a biblical scholar or as an adviser to the Swedish Academy on dictionary matters, and he only made one written foray into Indo-European philology. This apparently exists in a forty-page version, bearing directly on the law of the palatals, but is an elusive monograph. The full form, dated to 1878, is, however, unimportant; not only because of the lateness of the date but because we know (e.g. from 'document #4') that what was available in Lund at the relevant time, by Tegnér, was a printed fragment (sic) of five pages with a gummed-in sheet at the end explaining why the work was broken off. Apparently, what Tegnér wrote he wrote *after* the disclosures of October 1876 in Leipzig and January 1877 in Berlin. He lectured on the discovery in 1877 (see Mayrhofer 1983: 139). What and when his own earliest thinking was, and whether he was in mental communion with Thomsen in 1874–5, we cannot know. At best, we can allow that if Thomsen is the discoverer (despite a disclaimer at 1927 (1902): 94, which gives 1875 as the earliest relevant date), some prompting came from Tegnér. Yet must we leave the discovery in Thomsen's hands? Let us return to Verner.

Verner was modest and non-combative; but he was more interesting than that. He had something of the characteristic that the Scottish call being 'fey', of being subject to sudden almost miraculous flashes of insight, of putting two and two together where others do not see that two and two are even there. Remember the famous story of how he discovered the essential feature of his own law. He reminds one of Thomas Edison, who was no great scientist but who frequently realised what might be put with what to produce an invention. It is credible that the putting together of a problem of German voicing, Indo-European mobile accent, Indo-European ablaut phenomena, Indo-European cardinal vowel location –

that the connecting of these in a way that the pedestrian approach of Thomsen does not would be very much in Verner's line.

There is more. The law of the palatals has three interesting characteristics. First, of all the 'laws' of Indo-European (on which see Collinge 1985) only two were allowed at once to be correct and have never been overthrown or even seriously doubted. The law of the palatals is one, and Verner's law is the other. Secondly, secondary palatalization in Sanskrit looks like a body of inexplicable exceptions to normal reflexes. Verner made his name by his famous explanation of apparently inexplicable exceptions to the great regularity which we have long called Grimm's law. Thirdly, the law of the palatals takes a visible problem in Sanskrit and solves it by adducing critical evidence, the incidence of a front vowel, which is as such visible only in other languages. This is exactly Verner's technique in his own law, where he explains Germanic consonant forms (in terms of their voicing or non-voicing) by the positioning of Vedic, and hence Proto-Into-European and pre-Germanic, word accent. Frankly, if we could find somebody who knew everything about 19th century Indo-European studies and scholars but who had somehow not heard of the law of the palatals as such, and if that person were told of the law and asked to name a scholar most likely to have invented it, I am pretty certain he could only name Verner.

To summarize: Collitz, Thomsen and Verner remain in line for honour. Collitz did not discover the law first. But his clear, correct and courageous view of the Proto-Indo-European vowel system, in which one must have the guts (he says) to posit proper mid segments and which not even the laryngeal theory really disturbs, combines with his unerring linkage of the two problems in 1879. He had got there by early 1878, and nobody proves that he must have known the essentials of Scandinavian thinking before that. That is, he would have reached the same right result had no other done the same; and Allen's judgement needs only to be amended to 'a potential independent discoverer of the law'. Of the others, maybe Thomsen did get there first (in 1874 – or early 1875), but without a credibly operational vowel phonology and with some prompting from Tegnér; and then Verner's notions slipped into place as a result of Thomsen's conversation. Or else Verner had arrived at the solution in mid–1875 but, faced with Thomsen's chronological claim, had no stomach for a dispute or even for writing it all up. That would explain both his inability to check his personal enthusiasm in the Leipzig café and his embarrassment thereafter. For Osthoff (1886: 20) it is, finally, the law of Tegnér, Thomsen and Verner (with alphabetical tact). But at least until the chips are down – and with so many talks unrecorded and letters untraced the chips must stay up, probably for ever – this writer would give it to Verner.

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The second plural of the Umbrian verb

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1. Many of the verbs in the Iguvine Tables are singular active imperatives with the ending *-tu, -tu*, e.g. *enetu* Ia1 = *enetu* VIa1.* The etymology and value of this ending are clear. It corresponds to Oscan *-tud* in *líkítud* etc., Early Latin *-tōd* in *datod* etc., Classical Latin *-tō* in *datō* etc., Greek third singular *-τω* in *ἔστω* etc., Vedic *-tāt* in *bhavatāt* etc., and can be confidently reconstructed as Proto-Indo-European **-tōd*. (Whether this is originally an ablative of the pronoun **tó-*, as stated e.g. by Brugmann (1916: 572), is another matter, about which I have the gravest doubts.) Following recent usage, I will call imperatives made with the element **-tōd* or in analogy to it 'imperative 2', in contrast to 'imperative 1', which is formed without **-tōd*.

In Umbrian, as in Latin, the singular of imperative 2 functions as both second and third person. Second person is clear in the prayer texts, e.g.

VIa30 *futu fos pacer pase tua*

'be favorable [and] propitious with thy peace'.

Third person is clear in some of the instructions for performance of the rites described in the Tables. Nussbaum (1973) has adduced good reasons for thinking that the rituals described in Tables I and VI–VIIa were originally in the second person, and that this is preserved in Tablet I, while the redactor of Tablets VI and VIIa intended to transpose into the third person, but did not always succeed. A clear example where the change in person is signaled by a change in pronouns, while the verb itself remains unaltered, is Ib13 *steplatu* ... *tefe* 'require ... for yourself' vs. VIb51 *stiplatu* ... *seso* 'he shall require ... for himself'.

2. The corresponding mediopassive imperative 2 ending is *-mu, -mu*, e.g. the very frequent *pe(r)snimu, persnihmu, pe(r)snimu, persnih(i)mu* 'precator'. Here also the person is ambiguous between second and third, and in Tablet I *persnimu* etc. are second person and in Tablets VI and VII third person. This ending is clearly the Umbrian analog of the Old Latin deponent imperative 2 ending *-minō*, e.g. *prōgredīminō* Plautus, Pseudolus 859. Latin *-minō* (or already **-minōd*) was clearly formed to the second plural mediopassive indicative and imperative 1 *-minī* on the pattern of the active second and third singular imperative 2 *-tō(d)* beside second plural imperative 1 *-te* (and indicative *-tis, *-tes*). The rule would be 'form the singular of imperative 2 by replacing the nucleus (and coda, if any) of the final syllable of the plural

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of imperative 1 by *-ō(d)*'. In Oscan *censamur* 'he shall be assessed', Tab. Bant. 19, the voice has been redundantly marked by replacing the *-d* of **-mud* with the *-r* of other mediopassive forms such as *uincter* 'is convicted'; cf. the *-minor* of late Latin grammarians, Leumann (1977: 573).

Umbrian *-mu* and Oscan *-mur* are indirect evidence that corresponding to Latin *-minī*, Osco-Umbrian had second plural mediopassives with an ending of the shape **-mV(C)*; direct evidence are the mediopassive plural imperatives 1 *-mo*, 2 *-mumo* to be discussed in sections 3. and 4. Of the various explanations offered for Lat. *-minī*, the best in my opinion is the old one, that it is originally the nominative plural masculine of a participle in **-mVno-* (or **-mno-* with anaptyxis?), so that Lat. *sequiminī* is originally truncated from *sequiminī estis/este* (: ἑπόμενοί ἐστε, ἑπόμενοι ἔστε), used in place of the inherited second plural mediopassive ending Proto-Indo-European **-dhwe* or **-dhwo* which is found in Skt. *-dhve*, *-dhva(m)*, Hitt. *-ttuma(-)*, Luw. *-tuwa-*, and which probably underlies Gk. *-σθε*. If so, Oscan *-mur* and Umbrian *-mu, -mo, -mumo* imply for the immediate ancestor of Osco-Umbrian mediopassive participles in **-mo-*, of the type seen in East Baltic, Slavic, and Luwian (e.g. Lith. *mýlimas* 'loved', Latv. *jūtams* 'felt', OCS *iskušaemŭ* 'being tempted', Luw. *a-i-ia-am-mi-* 'made'). Whether these represent a formation distinct from the *-m(V)no-* of Lat. *-minī*, *fēmina, alumnus*, Gk. *-μενο-*, Av. *-mna-*, Skt. *-māna-*, Toch. B *-mane*, A *-māṃ*, OPru. *poklausīmanas* 'heard', or are all due to independent sound changes of **-mn-* to *-m-* is a problem I will not go into here.[1]

3. The Umbrian plurals of imperative 2 are active *-tutu* (Ib: 6 times), *-tuta* (III: 5 times), *-tuto* (VIb, VIIa: 12times), and mediopassive *-mumo* (VIb, VIIa: 5 times). The active forms here no doubt are the Umbrian analog of Lat. *-tōte*, i.e. are made by adding the active plural ending of imperative 1 to the active singular of imperative 2, on a pattern analogous to that of Latin *amā* : *amā-te* = *amātō* : X; X = *amātō-te*. (This was seen already by Brugmann (1892: 1327). Umbr. *-mumo* is the mediopassive pendant to this, i.e. is made by adding the ending of mediopassive plural imperative 1 to the mediopassive singular ending *-mu* of imperative 2 (as if Latin *prōgredīminō* had a plural *§prōgredīminōminī*).

As has often been noted, although the pluralizing elements of Umbr. *-tuto, -mumo* are etymologically second person only, these plurals are used also as third person, following the double value of the singulars *-tu, -mu*, in contrast with Latin, where the active of imperative 2 has a distinct third plural in *-ntō(d)*.

4. That the plurals of imperative 1 also are found in Umbrian was seen by Wackernagel 1907 (following a brief remark of Bücheler (1883: 197)). *Etatu* Ib 21, 22 = *etato* VIb 63 and *armanu kateramu* Ib 19f. = *arsmahamo caterahamo* VIb 56 are in a context that demands imperative 1: these are commands addressed to the men of Iguvium to be carried out at once, not instructions about how a rite is to be performed whenever the occasion for it arises, or prayers to deities asking them to behave in a certain way to the people of Iguvium or to their hostile neighbors. Wackernagel's insight was accepted by Brugmann (1911/1912 and 1916: 520, 577) and by Buck (1928: 362), and then forgotten until Rix (1976b: 225, n. 20, 1976c: 162, n. 10) restored it to the attention it deserves.[2]

On the origin of the ending *-to* Wackernagel and Brugmann disagreed. The latter thought it was from **-tā*, either **-t'ā*, i.e. the plural imperative ending **-te* elided before a particle **ā*, or the dual ending **-tā* seen in OCS *-ta* and Lithuanian *-ta*, refl. *-to-s* (so first Brugmann

(1892: 1327)). Wackernagel (1907: 318f.) thought it was **-to*, an *o*-grade of the second plural ending **-te* that appears e.g. in Gk. *-τε*, OCS *-te*. Brugmann (1911/12) defended his own view, Wackernagel (1911/12: 258f., n. 2) replied, and Brugmann (1916: 577f.) responded once more.

5. In my opinion, Brugmann's derivation from a dual **-tā* is more likely both phonologically and morphologically than Wackernagel's derivation from a plural **-to*. (Brugmann's alternative suggestion of a plural **-te* plus particle **ā* has nothing to recommend it.) Phonologically there is a difficulty for Wackernagel in the spelling *-tuta* of Table III in *ustentuta* 5, *upetuta* 10, *etuta* 11, *fertuta aituta* 13. While there is no serious difficulty in an ad hoc assumption that Proto-Indo-European final **-o*, of which there is no certain Umbrian example outside these endings, had simply remained, as **o* normally did elsewhere in Umbrian (cf. e.g. von Planta (1892: 109) for examples in initial syllables, and von Planta (1892: 567) for a possible example of final **-o* in *supu* IV 17), Wackernagel's derivation requires a further ad hoc assumption that final **-o* had undergone a lowering in articulation such that it could be written with ⟨a⟩ as well as with ⟨u⟩ in the native alphabet. But, as far as I see, all other clear or possible cases of **o* are written in the native alphabet only with ⟨u⟩, never with ⟨a⟩. Thus e.g. *pusnaes* Ia 2, *pustnaiaf* Ib 11 (cf. *postne* VIb 11); *ustentu* Ia 3 etc. (: *ostendu* VIa 20); *ukriper* Ia 5 etc., *ukar* Ib 7 (: *ocriper* VIa 23 etc., *ocar* VIb 46); *puplum* Ib 10 (: *poplom* VIIa 15). To suppose that final **-o* had undergone lowering to [ɔ] while non-final **o* did not is to introduce a complication into Umbrian historical phonology for which there is no evidence outside the second plural verb, and which has no support in the general phonetic tendencies of Umbrian and Osco-Umbrian.

On the other hand, Brugmann's derivation from **-tā* simply adds one more example for the development of **-ā* to [ɔ][3] that is already well known for Osco-Umbrian in such categories as the nominative singular of *ā*-stems, the nominative-accusative plural of neuter *o*-stems, and the active singular imperative 1 of *ā*-verbs. This sound is regularly written *o* in the Latin alphabet, but both *a* and *u* in the native Umbrian alphabet, e.g. accusative plural neutrum *arvia* Ia 3 etc., *aruvia* III 31, but *arviu* Ia 12 etc., *aruio* VIa 56 etc.; nominative singular *panta muta* Vb 2 but *entantu mutu* Vb 6; imperative 1 *stiplo* VIa 2, *aserio* VIa 4. The imperative plurals *-tu*, *-mu*, *-tutu* on Table I, *-tuta* on Table III, and *-to*, *-mo*, *-tuto*, *-mumo* on Tables VI and VII fit this pattern with no difficulty.

6. Morphologically also Brugmann's derivation is superior. Nowhere in Indo-European outside Umbrian is a second plural **-to* clearly attested. In languages that keep **-e* and **-o* apart, and where the second plural endings are not suspect of having dual origin, we find only **-te*, viz. Gk. *-τε*; *-te* or its reflexes in all Slavic languages, even those with *-mo* in first plural; Lith. *-te*, refl. *-tė-s*;[4] and probably Paelignian imperative *eite* 'ite', perfect *lexe* 'legistis', both Vetter (1953 no. 213); although I can cite no example of **o* in absolute final in Paelignian, note e.g. *iouiois puclois* Vetter (1953: 202), *suois cnatois* Vetter (1953: 203), *solois* Vetter (1953: 214), where the diphthong *oi* in final syllable is preserved, in contrast to the Latin development to *-eis* and then *-īs*. Cf. also the Irish imperative type *taibrith* Wb 6a 20, *léicid* Wb 6b 29, which on the evidence of the lenition after *bad* 'be ye' at Wb 24b 1 appears to come from plural **-te* rather than from dual **-tes*. Note also Hitt. *-tten*, *-tteni*, which, whether old plurals remodeled after first plural *-wen*, *-weni* or old duals with *-e-* from plural **-te* in place of the **o* implied by Gk. *-τον* and Toch. B third dual *-teṃ*,

normally have *e*, while the variant *-ttani* (a list e.g. in Kronasser (1965: 379ff.)) is more likely the result of Hittite accent placement on a preceding syllable than a relic of Proto-Indo-European (Proto-Indo-Hittite) ablaut. Although Tocharian A *-c*, beside which B *-cer* has an additional *-er* of unclear origin, could be phonetically from **-tes* as well as **-te*, I see no reason to suspect dual origin for this ending, so that Tocharian counts as another probable witness to second plural **-te*. The Old High German second plurals *beret, berat, birit* are a problem which I may treat elsewhere, but certainly do not require a Proto-Indo-European second plural **-to* (cf. Brugmann (1892: 1359; 1916: 625f.) for a number of old suggestions about these). Armenian *-yk'* in e.g. pres. ind. *sirēk', layk'*, impf. *sireik'*, pres. impv. *mi sirēk'*, pres. subj. *siric'ēk'*, aor. ind. *sirec'ēk', sirec'ayk'*, aor. impv. *sirec'ēk'*, aor. subj. *siresǰik'* certainly comes from **-te*(-) or **tē*(-), not **-to*(-), although I cannot say whether this is an old plural **-tě* remodelled with addition of *-k'*, or an old dual **-tes*, in which case the *-k'* may perhaps be a generalized sandhi variant (original before **w*-?) of **-s*.

Nor is there any raison d'être for a variant **-to* in this ending. In general, it is not licit to posit ablaut variants of the same morpheme without either data too clear to be ignored, or else a rationale by which the proposed ablaut clearly fits into a well established pattern with well established conditioning. Neither of these cases exists here. Wackernagel's appeal to first plural *-mos, -mo* beside *-mes, -me* (1912/13: 259) does not help, since the attested first plural forms can be explained on the basis of primary **-mos*, secondary **-me(-N)*, with the vowels distributed like in nom. **ékwos*, voc. **ékwe*, and with subsequent local creation of new variants *-mes* (Greek Doric *-μες*, poetic *-μεσθα*) and *-mo* (Serbo-Croatian, Slovene, Ukrainian, White Russian).

7. On the other hand, there are parallels for dual forms being generalized over plurals in languages that are losing the distinction of the two numbers. Within the first and second persons of the Indo-European verb, the clearest cases are probably the Latvian dialects with *-mā-s, -tā-s* instead of *-mie-s, -tie-s* as reflexive plural forms, and the Polish and Czech dialects with first plural *-wa* or *-ma* in place of standard Polish *-my*, Czech *-me* and second plural *-ta* in place of standard Polish *-cie*; for all these see Vaillant (1966: 27f.). But also the Old Irish second plural absolute of the type *be(i)the, ceste, gigeste* seems to require a Celtic ending **-tes*, not **-te*, and this **-tes* cannot be explained as analogic to the second singular in the way that Latin indicative/subjunctive *-tis* beside imperative *-te* can. It therefore seems to come from the Proto-Indo-European dual **-tes* of Sanskrit second person *-thas*, third person *-tas*, Avestan third person *-tō, -θō*, Gothic second person *-ts*, Old Church Slavic third person *-te* (perhaps with help from first plural **-mos*); cf. Cowgill (1975: 59f.). If so, Latin *-tis* itself may well be from dual **-tes*; and cf. section 6 above for the possibility of dual origin for second plural endings in Hittite and Armenian.

Another possible example for generalization of dual over plural is the Hittite first person plural endings, which are normally *-wen, -weni* (*-wani, -uni*) in active and *-waštа*(-) in mediopassive, and begin with *-m-* only when the preceding sound is *u*. If Hittite comes from a language which differentiated first plural endings in *-m-* from first dual endings in *-w-*, as Proto-Indo-European Proper certainly did (for this term see Cowgill (1979: 25)), then Hittite has clearly innovated by generalizing dual forms except after *u*. But, given the likelihood that the Indo-Hittite hypothesis is correct (Cowgill 1979), it is possible that Hittite comes from a protolanguage which had not differentiated first person plural from

first person dual by assigning different meanings to what were originally phonetically conditioned variants.

Within languages which had already lost a distinction between dual and plural in first and second person verbs, but retained distinct dual forms of first and second person pronouns, Bavarian and Modern Icelandic have generalized dual forms over plural in these pronouns: Bavarian only in second person *es, enk, enka*, Icelandic in both, *við, þið*, etc., beside which the old plurals *vér, þér* survive as honorifics; cf. Stefán Einarsson (1945: 68, 122). For similar phenomena in Faroese and in Norwegian and Swedish dialects cf. e.g. Bugge (1855: 247, 254ff.).

8. Latin and Irish seem to have used the contrast of plural **-te* and dual **-tes* to fashion a contrast between imperative and indicative, Lat. *agite* ≠ *agitis*, Prim. Ir. **berete* ≠ **-beretes*, vs. Gk. impv. λέγετε = indic. λέγετε, secondarily using the old dual **-tes* in all non-imperative tense/mood categories, even though outside present indicative there was no direct contrast with imperative – e.g. Lat. impf. *agēbātis*, subj. *agātis*, perf. *ēgistis*; Prim. Ir. subj. **-berātes*, fut. **gwigwessetes*.

From the use of dual **-tā* in imperative, it appears that Umbrian had no such contrast, at least in the active voice. And I suggest that the ending **-tā* is in fact found in Umbrian outside the imperative in the future perfects *couortuso* VIb 64 and *benuso* VIb 64, 65. (VIb 65 is repeated verbatim as VIIa 1–2.)

Nussbaum (1973: 366ff.) has very persuasively argued that these are second person active forms, not third plurals or passives (impersonals) as previously supposed, and proposes to take them as singulars, of deponent shape. In taking *benuso* and *couortuso* as second person, Nussbaum is undoubtedly correct, and has advanced greatly our understanding of these forms. Herewith is the relevant passage, as I punctuate it: (VIb 62–65): *Ape este dersicurent, eno deitu "etato Iiouinur" porse perca arsmatia habiest. Ape este dersicust, duti ambretuto euront. Ape termnome couortuso, sururont pesnimumo; sururont deitu, etaians deitu. Enom tertim ambretuto. Ape termnome benuso, sururont pesnimumo, sururont deitu etaias. Eno prinuatur šimo etuto erafont uia pora benuso.*

In literal, unidiomatic English I render this, 'When they will have said this [viz. the preceding prayer], then he that will have the *perca arsmatia* shall say, "Go(?), Iguvians". When he will have said this, the same ones (or, you same ones) shall circumambulate a second time. When you will have returned to the terminus, they (or, ye) shall pray in the same manner; he shall speak in the same manner, he shall say [that] they are to go(?). Then they (or, ye) shall circumambulate a third time. When you will have come to the terminus, they (or, ye) shall pray in the same manner, he shall say in the same manner [that] they are to go(?). Then the *prinuati* shall go back(?) by the same way by which you will have come.' Compare VIb 56f., describing the end of the first circumambulation: *Ape ambrefurent termnome benurent* 'when they will have circumambulated [and] will have come to the terminus', with both verbs in third plural active.

I have here deliberately rendered *couortuso* and *benuso* with the ambiguous English *you*, leaving it unspecified whether the verbs are singular or plural. Nussbaum takes them as singular, referring to the individual to whom the instructions for the lustration ritual in its original formulation were addressed generally. (Cf. Ib 10 *pune puplum aferum heries* = VIb 48 *pone poplo afero heries* 'when thou wilt want to circumambulate the people'.) That is

certainly possible. But in each case at least three people are doing the moving – "he who will have the *perca arsmatia*", and the two *prinuati* (*prinuatur dur* VIb 50). Hence the plural verbs *ambretuto, pesnimumo*, and the *termnome benurent* of VIb 57 corresponding in the third person to the *termnome couortuso* and *termnome benuso* of VIb 63f. and 64.

Pragmatically, therefore, it is at least equally easy to take *couortuso* and *benuso* as plurals, referring to the entire group making the lustration, and, at least in the original formulation, agreeing in person and number with the plural imperatives *pesnimumo*:

63f. *Ape termnome couortuso, sururont pesnimumo*
'when ye will have returned to the terminus, ye shall pray in the same manner';

64f. *Ape termnome benuso, sururont pesnimumo*
'when ye will have come to the terminus, ye shall pray in the same manner'.

In 65, *prinuatur sìmo etuto erafont uia pora benuso*
'the *prinuati* shall go back(?) by the same way by which ye have come'

evidently correlates with

VIb 52 *uia auiecla esonome etuto*
'they (viz. he who will have the *perca arsmatia* and the two *prinuati*) shall go to the ceremony by the augural way.' Contrast the formulation in Ib 20f. *Puni amprefu(u)s, persnimu* 'when thou wilt have circumambulated, thou shalt pray', where the clear singular *amprefu(u)s* is correlated with the clear singular *persnimu*.

9. Nussbaum thinks that *couortuso* and *benuso* are deponent in form, with the second singular secondary ending **-so* of Greek (e.g. Hom. δέξο), Iranian (e.g. OP *yadaišā*), and Latin (*-re* in all tenses and moods, sometimes extended to *-ris* outside the imperative, *-rus* in some dialect forms). He supposes that the perfects of **g^{w}em-* and **wert-* were originally deponent in Umbrian, before being remodeled to the active shapes attested as *benus* IIb 16, *benust* VIb 53, *benurent* Va 25, 28, Vb 5 *benurent* VIb 57 and *vurtus* IIa 2, *kuvurtus* Ib 11, *couortus* VIIa 39, *courtust* VIa 6. When Tables VI and VII were inscribed he thinks that *couortuso, benuso* were not replaced by the new active forms either because the engraver did not understand them and so left them unchanged, or because he interpreted them as impersonals with an ending *-o(r?)*.

Phonologically, there is no difficulty with this view. The objection in section 5 to Wackernagel's derivation of the imperative plural *-tu, -ta, -to* from **-to* does not apply here, since there are no forms in the native alphabet of the type *§benusa, §kuvurtusa*.

But morphologically I think the difficulties are considerable. Non-periphrastic deponent inflection is quite unknown elsewhere in the Osco-Umbrian perfect. In Latin, where we know the full system, we can say that the non-periphrastic perfect was activum tantum (on the active origin of the first sg. *-ī* cf. e.g. Szemerényi (1970: 226), and on the third pl. *-ēre* from **-ēri* see Peters (1981: 102)). Indeed, Latin even has a few active perfects correlated with deponent presents, viz. *revertī* : *revertor, meminī* : *reminīscor* (Leumann 1977: 509). Latin deponent perfects are formed with copula plus *to*-participle, just like passive perfects, e.g. *nactus sum, ausus sum* like *captus sum*. The reason for the lack of non-periphrastic mediopassive perfects in Latin is presumably that the Proto-Indo-European perfect lacked a distinction of voice, and Latin never created a distinct mediopassive perfect (unlike Indo-Iranian, Greek, and Celtic), instead conforming its preterits of aorist origin to its perfects

by abandoning mediopassive inflection in such stems. (But not in the type *iussitur, mercāssitur*, which was not incorporated into the perfect system.)

Aside from *couortuso, benuso* the extant Umbrian material conforms to Latin, with exclusively periphrastic mediopassives within the perfect system, whether passive like *ukar pihaz fust* Ib 7 = *ocar pihos fust* VIb 46f. 'the citadel will have been ritually cleansed', or deponent like *pue persnis fust* VIb 39 'who will have prayed'. (Cf. Rix (1976a) on the correct analysis of forms in *-f(e)i* as present passive infinitives.) Oscan does seem to have at least one non-periphrastic passive within the perfect system, *comparascuster* 'will have been discussed' Tab. Bant. 4. (I omit discussion of such problematic forms as *lamatir* Tab. Bant. 21, *lamatir* Vetter 6.4, *sakrafir* Vetter 86.9f., 87.9.) But this is passive in value, not deponent, and is an easily understandable innovation (as if Latin were to form *§cēperitur* beside *captus erit*); and Oscan also has periphrastic passive perfects and pluperfects, e.g. *lígat[ús] fufans* 'had been delegated' Cip. Ab. A9f., *prúftúset* 'have been approved' ibid. A16, *scriftas set* 'have been written' Tab. Bant. 25. (But *ancensto fust* ibid. 22 is probably adjective plus copula 'will be (in) unassessed (state)', not periphrastic future perfect 'will not have been assessed'.)

It is therefore not easy to suppose that Umbrian once had deponently inflected future perfects to the active presents attested in *menes* 'thou wilt come' Ib 15 and *kuvertu* Ib 9 etc. *couertu* VIb 47 etc. 'thou shalt/he shall return'.

10. If we take *benuso, couortuso* as active plurals, the difficulty of explaining how Umbrian would have the mediopassive ending **-so* in its future perfect disappears. And we can suppose that the mistake that the redactor of VI and VII made was simply that, misled by the ambiguous neighboring *ambretuto, pesnimumo, etuto*, he failed to change second plural *couortuso, benuso* into third plural **couorturent, benurent* (as he remembered to do at 56f. *ambrefurent . . . benurent*), i.e. exactly the same mistake that Nussbaum has so convincingly demonstrated for the singulars in *-us* instead of *-ust* in VI and VII.

But a new difficulty arises. Although the origin of the Osco-Umbrian future perfect formant *-us-* is obscure, it at least combines regularly with the personal endings second singular *-s*, third singular *-t*, third plural *-ent* to give Umbrian *-us, -ust, -urent*; cf. Oscan third singular *dicust* etc., third plural *tríbarakattuset, angetuzet*, pass. *comparascuster*. I would therefore expect *-us-* to combine with the second plural ending *-to* < **-tā* to give *§-usto*, not *-uso*. Oscan and Umbrian have numerous cases of *st* which apparently go back to Proto-Indo-European **st*, e.g. Umbrian *pustnaiaf* Ib 22, and it does not seem feasible to explain all of these by analogy or special sound laws.

However, the difficulty of an ending with *-s-* for expected *-st-* in Osco-Umbrian is not unique to Umbrian *couortuso, benuso*. In the Paelignian inscription Vetter 213, Thurneysen long ago (1888: 352) proposed to take *lexe* in line 7 as the equivalent of Lat. *lēgistis*, i.e. a second plural perfect in which the ending *-te* had somehow combined with the Paelignian equivalent of Lat. *-is-* to give *-se*. Thurneysen thought of simplification of the consonant cluster *xt* of **lexte* syncopated from **légiste*. But von Planta (1897: 283) pointed out that we might expect **lekste* to be simplified rather to *§leste*.

It looks instead as if, for reasons that now escape us (partly because our knowledge of the languages is so fragmentary), the *-t-* of the second plural endings *-to* and *-te* combined in Osco-Umbrian with a preceding *-s-* to give an *-s-* that was, apparently, short (at least in

Umbrian), but differed from inherited **s* in not being subject to rhotacism in Umbrian. How this came about, I cannot say, but Paelignian *lexe* and Umbrian *benuso, couortuso* support each other. As often in historical linguistics, to the theory and methodology of which Henry Hoenigswald has contributed so much, two parallel cases, each of which by itself would be rather dubious, gain greatly in verisimilitude by the very fact of their being two. (As von Planta (1897: 366) points out, is is not clear whether *lexe* is a suffixless preterit like Lat. *lēgī* or an *s*-preterit like Lat. *intellēxī*; in the former case, it is evidence for a stem extension *-s-* or *-Vs-* in the second plural perfect in Osco-Umbrian parallel to the *-is-* of Lat. *lēg-is-tis.*)

11. It appears therefore that in the distribution of the Proto-Indo-European second person endings plural **-te* and dual **-tes, *-tā* the different subbranches of Italic and Celtic have gone very different ways. Latin and Irish seem to have retained plural **-te* as a distinctive imperative ending, while using dual **-tes* for the other moods. Paelignian, on the evidence of imperative *eite* and indicative *lexe*, generalized plural **-te* in all moods, while Umbrian generalized dual **-tā* in all moods. For Oscan proper and Continental Celtic we lack evidence,[5] and the British languages have undergone too much change for us to say which of these three endings their second plurals continue.

The shapes and distribution of the second and third person dual endings in Proto-Indo-European remain unclear. Umbrian adds a witness to Balto-Slavic for a second dual in **-tā*, but the original distribution and antiquity of this vis-à-vis the **-tes* of Irish, Latin, Gothic, Slavic, and Indo-Iranian remains problematic, as well as its relation to the secondary third dual **-tām* of Sanskrit and Greek, and of all these to the **-tom* of Greek *-τον*, Indo-Iranian *-tam*, Toch. B. *-teṃ*.[6] (Quite unclear the Tocharian preterit third person duals B *stāmais, ltais*, A *tākenas*, except that, like all Indo-European active second and third person duals, these can be described as second plural endings with additional marking: in this case, the second plural endings are those of the Tocharian preterit and imperative, type B *rańkas, pkarsas*, A *kotas, ptärkās.*)

A final note on the non-imperative second plural mediopassive of Umbrian. If the imperative *-mo, -mumo* has been (re-)shaped on the model of active imperative *-to, -tuto* (as if Lat. impv. *-minī* had been replaced by §*-mine* on the analogy of active imperative *-te*), and if the active has *-to* in indicative *benuso, couortuso* as well as in imperative, then there is every reason to suppose that the second plural indicative and subjunctive mediopassive ending of Umbrian also remodeled its original shape (**-mu(s), *-mur* from nominative plural masculine **-m(n)ōs?*) to **-mo* (as if Latin indicative/subjunctive *-minī* has been remodeled to **-minis* after active *-tis*).

Notes

[1] Prosdocimi (1979: 212) proposes to restore]*ṇod* in an early inscription from the neighborhood of Tivoli, with an ending analogous to Umbrian *-mu*, but in a Latin dialect. While the final *-d* is just what we expect in Early Latin, *-m-* instead of *-m(V)n-* in a Latin dialect would be very surprising, and I wonder if it is possible to read the traces of the first visible letter as *n*, giving an imperative in *-m(V)*]*ṇod*. On the likelihood that Luwian *-mmi-* and Balto-Slavic **-ma* reflect **-mno-* see H.C. Melchert (*Die Sprache* 29. 1983: 23–25).

[2] I am not sure enough about the meanings of *eta-*, *arsma-*, and *catera-* to venture translations for these imperatives. *Eta-* is generally taken as equivalent to Latin *itā-* 'be in the habit of going', but a frequentative seems out of place here, nor is it clear to me that the meaning 'go' fits. The command *etato* (subj. *etaia(n)s* in indirect discourse VIb 64, 65 (=VIIa 1)) is addressed to the men of Iguvium after the prayers following each of the three circumambulations of the lustration. It cannot well mean 'go (away, because the ceremony is over)' – that would be appropriate only at the end of the part of the rite that involved the men of Iguvium generally. It is scarcely likely that the men of Iguvium are to go away three times during the ceremony, presumably returning at least twice, and nothing is said about a return of the men of Iguvium after these putative departures. Nor, as far as I understand the text, do the men of Iguvium as a whole take part in the circumambulation; rather they remain in the same spot, and 'he who has the *perca arsmatia*' and the two *prinuati* do the circumambulating. And the command *etato/etaians* is given after each of the three circumambulations, not before.

[3] Lejeune (1949: 107) has shown that this vowel was probably shortened in Umbrian, as it evidently was also in Oscan (Lejeune 1949 passim).

[4] I leave undecided whether this is an old variant **-tē*, or represents analogic remodeling after forms which retained inherited long vowels and diphthongs before enclitics and shortened them in absolute final.

[5] Léon Fleuriot has suggested (*ÉC* 17. 1980: 135), that Gaulish *loncate* is a second plural indicative 'vous avalez'; and one year later (Fleuriot, *ÉC* 18: 1981, 91–92) he has suggested that Gaulish *ibetis* and *biiete* are second plural imperatives, 'buvez', 'soyez', the former with an enclitic pronoun *-s*. If these interpretations are correct, Gaulish, like Paelignian and Greek, but unlike Latin, Irish, and Umbrian, preserved Proto-Indo-European plural **-te* in both indicative and imperative, without intrusion of dual forms.

[6] The *-ṃ* here is of course not the **-m* of the Proto-Indo-European ending, which after **o* would have been lost without trace in Tocharian, cf. e.g. B *ce* 'hunc'. It is presumably the same third person pronoun as in the B third singular present *nesäṃ* etc.

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heres, χηρωσται: indogermanische Richtersprache

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> "*Alter familienrechtlicher Ausdruck, der Bildung nach mit* ὠμηστής *vergleichbar*" H. Frisk

1. Pott: **ghed-*

In 1867, A.F. Pott (1867: 68–71) compared Greek χήρα 'widow' with Latin *heres* 'heir', and 'sniffed out' (*witterte*) in the latter an old compound, with a verbal second element.[1]

As the first member, Pott saw a **-ro-* derivative of the root **ĝheE-* 'abandon' (Pokorny 1959: 418), as in χήρα and the later χῆρος 'widower; bereaved, empty'. This much of his analysis has remained generally accepted, although **-ro-*derivatives generally show the zero-grade of the root (Brugmann 1906: 348–50).

The identity of the second element has been more controversial. Pott considered three possibilities: the root **Eed-* 'eat' (Pokorny 1959: 287ff);[2] an Indo-European form related to Indic *á dā-* 'take'; and the root **ghed-* of Lat. *praehendo*, Engl. *get*, etc. (Pokorny 1959: 437–438). Pott thought the last alternative to be the simplest and most likely; his derivation, though not stated as such, was **ĝhēro-ghed-* > **hēro-hed-* > *hērēd-*.

2. Prellwitz: **Eed-*

But this was rendered impossible by Prellwitz's adduction of the archaic Greek legal term χηρωσται. Assuming in the latter a secondary recharacterisation by the agent-suffix **-tā-*, proto-Greek **khērōd-* and Latin *hērēd-* together permitted the reconstruction of an ablauting, athematic **ĝhērē/ōd-*. Prellwitz saw in the second syllable the verbal root **Eed-* 'eat', but as support offered in the first place citations from Horace.[3] This seems to have convinced no one, with the single exception of M. Leumann (1977: 391, 393).

LSJ define χηρωσται as 'far-off kinsmen, who seize and divide among themselves the property of one who dies without heirs (χῆρος)'. The two early hexameter attestations, namely

Il. 5.155–8 ἔνθ' ὅ γε τοὺς ἐνάριζε, φίλον δ' ἐξαίνυτο θυμὸν
ἀμφοτέρω, πατέρι δὲ γόον καὶ κήδεα λυγρὰ
λεῖπ', ἐπεὶ οὐ ζώοντε μάχης ἐκ νοστήσαντε
δέξατο· χηρωσται δὲ διὰ κτῆσιν δατέοντο.

and, with enjambement,

Theog. 604–7 ...ὀλοὸν δ' ἐπὶ γῆρας ἵκηται
χήτει γηροκόμοιο· ὁ δ' οὐ βιότου γ' ἐπιδευὴς
ζώει, ἀποφθιμένου δὲ διὰ ζωὴν δατέονται
χηρωσταί·...

are paradigms of the dangers of lack of sons, a well-known worry of e.g. the Vedic Aryans as well. In both passages, a dearth of direct descendents, due to slaughter or celibacy, leads to a man's property being divided up (*διὰ δατέομαι*) by the *χηρωσταί*.[4] Although it is not unlikely that the *χηρωσταί* were distant relatives, nothing in the texts supports this view at all; the paradigms become all the more horrible if outsiders, rather than kinsmen, divide up the 'abandoned' inheritance.

3. Brugmann: *á dā-*

Recent scholarship[5] has generally followed Brugmann's resolution of Pott's triple choice (Brugmann 1904: 30; 1906: 79, 396f.). For him **Eed-* as second element, in a compound meaning 'das Ererbte verzehrend', was as impossible for semantic reasons as was **ghed-* phonologically, since, as Pott had put it, such a metaphor "hiesse dem ernsten, und zumal in Rechtssachen keinen Spass verstehenden Römer einen übel angebrachten Witz unterlegen" (Pott 1867: 69, cited approvingly by Brugmann 1904: 30).

Brugmann was thus left with Pott's third etymological possibility: connection with Indic *á dā-* 'take'.[6] The original meaning would thus have been 'Erbempfänger', as in Skr. *dāyādá-* (Manu+), cited already by Pott. And a European equivalent to the Indo-Iranian preverb **á* would have been ascertained.

Although I would share Brugmann's joy at finding a clear European counterpart to I–Ir. **á*, I doubt that **ĝhērē/ōd-* contains anything of the sort. Despite the near-universal acceptance of Brugmann's analysis, various problems remain unresolved. Formally, what happened to the final consonant of the root, i.e. the **-O* in **deO-*? Brugmann spoke of loss before vocalic endings in the paradigm, but this means taking Lat. *hered-* as only secondarily athematic in the nominative – rather unlikely in view of Gk. **khērōd-*. Brugmann further compared the Indo-Iranian 'infra-zero-grades', as in 1st pl. pres. act. *da-d-más*, vbl. adj. *á-t-ta-*, cpd. *devá-t-ta-*. Greek **-ōd-tās* from **-o-ō-dO-teA-* would be the only instance in Greek of such syllabic loss.[7] But despite the constant progress of laryngeal studies, this is still more of an 'obscurum per obscurius' than an explanation.[8]

The preverb also poses problems. Frisk and Beekes (1975: 9f.) have rightly objected to the supposed presence of both ablaut forms, **é* and **ó*, in the same word. Furthermore, the sense 'take' of Indic *á dā-* is due in my opinion to the diathesis alone (exclusively middle in the Rigveda), not to the preverb.[9] Other semantic problems are noted in section 11.

4. Beekes: suffix **-ed-*

Beekes, who rejected **Eed-* as second member "on account of the meaning" and an *á dā-* equivalent for the apophonic reasons mentioned above, has recently suggested a return to another old belief: that the second syllable was not a root at all, but a suffix (Beekes 1975: 9f.).[10] Starting from an original nom. **ĝhéEr-ōd(-s)*, acc. **-éd-m̥*, gen. **-d-ós*,[11] varying generalisations would account for the disparities between the Greek and Latin vocalisms.

This proposal, cited approvingly by Chantraine, is indeed a plausible resolution of the diverging vowels – but of little else.[12] First, the comparative evidence for a suffix **-ed-* is extremely limited: the only other instance provided[13] was I–Ir. **śarad-/śard-* 'autumn', itself very obscure.[14]

Further, Beekes made no mention of the value of the suffix in his reconstructed **ĝheEr-od-*. Is it possessive, agentive, or something else? Our delight in powerful new paradigmatic models does not free us from the need to explain the function of the reconstructed morphemes.[15]
Finally, a segmentation **ĝheEr-od-* separates the first element from the thematic **-ro-* derivatives of Greek; this too needs explanation.

In any case, we could still accept Beekes's ablaut, if we wanted,[16] without feeling obliged to avoid the semantic question as well.

5. My view is that only the derivation from **Eed-* 'eat' is correct. I believe in the utter exactitude of both clauses of Frisk's initial sentence quoted at the outset.[17] Proof comes from the texts, rather than from considerations of ablaut or paradigms. Still, let us note at the start that Frisk's formal objection to this interpretation, "gegen Anknüpfung an *ed-* spricht . . . der *ω*-Vokal", is groundless. The root **Eed-* is known to occur in both long- and full-grades (Narten (1968: 15, n. 44); differently Watkins (1969: 31–32)), both *e-* and *o*-colored (Schindler (1975: 61ff.)). Regardless of which explanation of the variant forms appeals to us most, allomorphy will not prevent us a priori from finding a form of **Eed-* in our word.

My suggestion is to begin from **ĝheEro-Eed-*. The dialectally varying contraction-colors are comparable to those in the thematic ablative singular: **-o(A)ad* gave **-ôd* in Latin, Greek, and Celtiberian (see Untermann (1967: 281ff.)), but **-ād* in Baltic.

A pre-form **ĝheEro-Eed-* is morphologically plausible because, in contrast to the isolated and meaningless "suffix **-ed-*", a small cluster of parallel verbal government-compounds with **Eed-* as second member is traceable. Most obvious is **ōmo-Eed-*, as preserved in RV. *āmād-*,[18] Gk. *ὠμησ-τής*[19] (with the same recharacterisation in **-tā-* as in *χηρωστής*); perhaps old is also **medhu-Eed-*, as in RV *madhvád-*[20] and, with long-grade and substantivised, OCS *medvědı̀ 'ἄρκτος'*. All but the last are found as epithets of birds[21] and carrion-eaters.[22]

6. Related phraseology: Greek

The Odyssey describes an attempt at usurpation of an inheritance by non-kinsmen who ignore a legitimate heir's right to his property, and, what is more, plot to kill him. The suitors' activity is metaphorically termed 'eating the house'. An undertone of disapproval and blame accompanies this violent image:

1.250–1	*... τοὶ δὲ φθίνουσιν ἔδοντες*
	οἶκον ἐμόν· τάχα δή με διαρραίσουσι καὶ αὐτόν.
2.237–8	*σφὰς γὰρ παρθέμενοι κεφαλὰς κατέδουσι βιαίως*
	οἶκον Ὀδυσσῆος...
4.318	*ἐσθίεταί μοι οἶκος, ὄλωλε δὲ πίονα ἔργα·*
16.431	*τοῦ νῦν οἶκον ἄτιμον ἔδεις...*

21.332–3 ...οἳ δὴ οἶκον ἀτιμάζοντες ἔδουσιν
ἀνδρὸς ἀριστῆος....

Feast imagery, referring to the suitors' gluttony, is of course widespread all through the Odyssey,[23] and at times is broadened to include consumption of 'the wealth' in general:

11.116 ἄνδρας ὑπερφιάλους, οἵ τοι βίοτον κατέδουσι
13.419 ...βίοτον δέ οἱ ἄλλοι ἔδουσι.
14.377 ἠδ' οἳ χαίρουσιν βίοτον νήποινον ἔδοντες·
17.378 ...βίοτον κατέδουσιν ἄνακτος

and, with other direct objects,[24]

16.389 μὴ οἱ χρήματ' ... ἔδωμεν
23.9 ...κτήματ' ἔδον βιώντό τε παῖδα.

With the suppletive aorist stem φαγ-:

3.315–6 ...μὴ τοι κατὰ πάντα φάγωσι
κτήματα δασσάμενοι...
16.428–9 τὸν ῥ' ἔθελον φθῖσαι καὶ ἀπορραῖσαι φίλον ἦτορ
ἠδὲ κατὰ ζωὴν φαγέειν μενοεικέα πολλήν·

In this wider context, we might wonder how much of the feast imagery is the Odyssey-poet's own contribution. My opinion is that since only the expression 'eating the house' is found as well in related traditions in the same meaning (dishonorable, potentially violent appropriation of property), only it is inherited; the rest we can ascribe, for the moment at least, to the elaborating hand of the monumental Odyssey-poet.[25]

7. Related phraseology: Hittite

The Odyssean house-eating expression is paralleled in one of the oldest Hittite texts: the proclamation of Telipinu (*CTH* 19). This attempt at regulation of royal succession begins with a detailed catalog of internecine murders and usurpations. I. 21–2 is a passage which has long caused problems for Hittitologists:

mān appizziyan=ma ÌR.MEŠ DUMU.MEŠ LUGAL *maršeššir*
nu É.MEŠ-*ŠUNU karipuwan dair*
išḫašaš=ma=šan taštašeškiuwan dair
nu ešḫar=šumit eššuwan tier

"Als aber später[26] die Knechte der Prinzen betrügerisch wurden, begannen sie, ihre Häuser zu fressen, gegen ihre Herren immer wieder Verrat zu üben und (immer wieder) ihr Blut zu vergiessen." (Eisele (1970: 21)).

The most recent editor, Eisele, calls the passage "nicht ganz klar", and offers two possible interpretations: either (following Friedrich) the servants destroyed the princes' houses, or (following an oral suggestion of Sommer), "auffressen = Verwirtschaften der Güter, die die Knechte für die Prinzen bewirtschafteten, zu eigenen Gunsten." (Eisele (1970: 73); I quote here from Kammenhuber (*MIO* 3. 1955: 34ff.)). Similarly Hoffmann (1984: 17).

But the similarity with the Odyssean metaphor is unmistakable, despite the lexical replacements (*pir/parn-* (= É) for **dom-* or **wiḫ-* continuants; animalistic, 'daevic' *karep-*[27] rather than *ed-*). Corresponding to the suitors we have the 'princes' servants' who begin to 'devour' their masters' 'houses'. The sense must be that the servants began to intrigue so as to usurp royal property and thereby royal power.[28]

8. Related phraseology: Rigveda

The Rigveda too is full of references to division of wealth, though usually in the form of prayers for a share in the booty taken from a defeated pagan enemy. In one stanza, however, eating terminology must refer to family property. These mantras occur in a series of riddle-verses addressed to Indra:

2.13.4abc *prajā́bhyaḥ puṣṭím vibhájanta āsate*
rayím iva pṛṣṭhám prabhávantam āyaté
ásinvan dáṃṣṭraiḥ pitúr atti bhójanam

'They sit, distributing wealth to the offspring — riches like a peak, prominent to one who approaches. *ásinvant-* with (his) teeth, he eats the wealth of the father.'[29]

Both *puṣṭí-* and *bhójana-* can mean 'food' as well as 'wealth' in the Rigveda. And food and wealth are mixed up in the complex simile in *b*.[30] But formulaic analysis shows that both must be taken in a non-alimentary sense here: for the first, cf. our initial alliterative formula in another wealth context:[31]

8.59.7bc *rāyás póṣaṃ yájamāneṣu dhattam*
prajā́m puṣṭím bhūtim asmā́su dhattam

For the second, cf. the common formula which is split between verses *a* and *c* in our passage:

2.26.1d *yájvéd áyajyor ví bhajāti bhójanam*
10.48.1d *ahám dāśúṣe ví bhajāmi bhójanam*

In fact, *ví bhaj-* is the term normally used of disposition of the wealth of the pagan enemy, as in

10.84.2c *hatvā́ya śátrūn ví bhajasva védaḥ*
10.27.10d *áyuddho asya ví bhajāni védaḥ*
1.103.6d *áyajvano vibhájann éti védaḥ*
7.32.7c *ví tvā́hatasya védanam bhajemahi*, etc.[32]

Functionally, *ví bhaj-* corresponds to διὰ δατέομαι, the characteristic activity of χηρωσταί. Further, διὰ and *ví* are etymologically related (*KZ* 95 (1981): 230), while *bhaj-* is etymologically cognate with the Greek stem φαγ-, forming an aorist to ἐσθίω. The language seems old.

The wealth being eaten is characterised as *pitúr*, 'of the father'. Here I must reject Bergaigne's artificial mythologic explanation, accepted by Oldenberg,[33] in favor of the simple interpretation that a father is the recent decedent, cf.

1.70.10d *pitúr ná jívrer ví védo bharanta*

In other words, we are dealing here not with the booty of pagan enemies, but with a familial disposition. The unexpressed subject of *āsate* in *a* is something like 'the family, the clan'.

The meaning of *ásinvant-* is unclear,[34] although given the riddle context, Geldner's "ohne Bissen zu machen ißt er mit den Beißzähnen" sounds plausible.[35]

The general picture is thus that of a ceremonial distribution of a dead elder's wealth among his offspring. The high pile beckons, however, also to the approaching potential usurper, who can 'eat' without chewing. We recognise a now-familiar use of *ad-*.

9. Evaluation of related phraseology

In short: early Greek, Hittite, and Vedic texts speak of 'eating' or 'devouring' property in reference to the usurpation of a household by others than a direct son of the 'pater familias'. This seems indeed to be an "alter familienrechtlicher Ausdruck".

This idiomatic, metaphoric phraseology shows that the verbal root **Eed-* is in fact a perfectly plausible second member of **ĝheEro-Eed-*. The original meaning could only have been 'who eats what has been abandoned', i.e. the house and property of the departed father.[36] Rather than causing semantic problems, recognition of **Eed-* 'eat' in this compound fits well into the violent, extra- or marginally-legal semantic sphere of the usurpative phraseology.

10. Animal imagery for anti-social acts

More support for this interpretation comes from a more general consideration of animal imagery referring to disruptive behavior in early dialectal texts. As is known, the direct naming of dangerous or annoying animals (wolf, bear, ant, wasp) was avoided in polite Indo-European conversation, on the *nomen est omen* principle. This led to circumventive strategies such as taboo deformation, replacement by an epithet, and other periphrases.

In extreme instances of asocial behavior, however, the breaking of this taboo seems to have been societally sanctioned. E.g. in wartime, an accepted feature of heroic conduct was insult before combat. This could include the mocking, proleptic description of the feast that vultures, fish, dogs, worms, or other flesh-eaters would make of the opponent's body as it lay on the ground. For the moment,[37] cf.

RV 10.95.14cd *ádhā śáyīta nírṛter úpasthe*
'dhainaṃ vṛ́kā rabhasā́so adyúḥ

'There would he lie in the lap of death, there would ravenous wolves eat him', with

Il. 22.42–3 ...τάχα κεν ἐ κύνες καὶ γῦπες ἔδοιεν/κείμενον...

In particular, the taboo on the invocation of the wolf seems to have been suspended to mark rejection from the clan after destructive behavior or crime. E. Campanile has shown that "in the juridic language, 'wolf' was the technical denomination of the man banished from his people" (*JIES* 7. 1979: 237ff.) by comparing phraseology from Hittite,[38] Indic,[39] Germanic, and Celtic. Cf. also Livy 1.4.7 *sunt qui Laurentiam vulgato corpore lupam inter pastores vocatam putent.* The reason for the suspension of a taboo so strong that it seems to have led to metatheses and dissimilations already in Indo-European can only have been the extreme usefulness of the animal imagery in referring to censurable behavior.

Now **ōmo-Eed-* and **medhu-Eed-*, we remember, occurred as epithets of predatory birds and other animals (section 5). So too, the continuants of **ĝheEro-Eed-* and related expressions were also closely associated with gore, as in Odyssean τάχα δή με διαρραίσουσι καὶ αὐτόν, σφὰς γὰρ παρθέμενοι κεφαλὰς κατέδουσι βιαίως/οἶκον, φθῖσαι καὶ ἀπορραῖσαι, etc.;[40] and Telipinu *nu eš̆har=šumit eššuwan tier; karep-* 'fressen' for *ed-* 'essen', etc.

In short: animal imagery was used to refer disapprovingly to disruptive, disorderly behavior, and the **-Eed-*compounds formed part of this animal imagery.

11. Connotations of *ĝheEro-Eed-

Although Latin *heres* indicates the heir in general, χηρωσταί are something rather more specific: appropriators of a vacant inheritance. Benveniste has suggested that Indo-European possessed no general word for heir at all. Such a term was not needed in a patriarchal society, where property normally passed only from father to son. But a term might have been useful for deviations from this practice.[41]

ĝheEro-Eed- seems to have been an accepted metaphor[42] of the *indogermanische Richtersprache* for collateral heirs and usurpers of households outside the direct line of descent. Brugmann and Benveniste, with their bland 'Erbempfänger' and 'héritier', back-project a later, more abstract legal system into Indo-European times. They ignore as well the animalistic semantic component suggested by related phraseology and by the form's compositional cluster-mates. Far from jocular, the connotation of *ĝheEro-Eed-* seems to have been that of a near-taboo, asocial scavenger of "abandoned" property. Time and social change have caused semantic fading: to "collateral heir", then to "heir", and even to "relative" in general, if Hesychian οἱ μακρόθεν συγγενεῖς is to be taken seriously.

The presence of metaphor, a feature of the *Dichtersprache*, in the *Richtersprache* as well is not surprising. Formulaic expressions, no-longer-understood archaisms beside up-to-date expressions, parallelisms of structure, and other seemingly oral poetic features are common in legal texts, and the relations between law and poetry have been repeatedly explored. Metaphor is in any case a constantly self-renewing feature of human language in general: though highly developed in, it is by no means limited to a poetic style.

Notes

[1] I thank the Alexander von Humboldt-Stiftung, my university, and my department for the stays in Würzburg (1980–1981) and Paris (1982–1983) during which this paper came into being.

[2] On the laryngeal see Schindler (*KZ* 89. 1975: 61ff.); but the argument of this paper is in no way dependent on the laryngeal.

[3] Cf. Prellwitz (*BB* 25. 1899: 313–314). He did however also mention the compound-cluster discussed in section 5.

[4] For normal distribution among living direct sons, cf. *Od.* 14.208–9 *...τοὶ δὲ ζωὴν ἐδάσαντο / παῖδες ὑπέρθυμοι καὶ ἐπὶ κλήρους ἐβάλοντο.* The δατέομαι phrases all fill the line from the masculine caesura onwards.

[5] Boisacq (1938), Walde-Hofmann (1938–1956), Schwyzer (1939–1950), Pokorny (1959–1969), and Frisk (1960–1972) *s.v.*; Ernout-Meillet (1951–1952) reject summarily all explanations of the second element, without proposing one of their own.

[6] Which he considered to have been the ultimate source of his **ed-* 'eat' anyway (Brugmann 1904: 31)!

[7] Nothing comparable is mentioned by Szemerényi (1964).

[8] None of the explanations of the Indic infra-zero-grades (e.g. generalisation from 3rd pl. **de-dO-nti* (Meillet 1922: 66); automatic non-distinctive overlength-avoiding syllabicity (Hoenigswald 1965: 97–99)) will work for χηρωστής.

[9] More on the preverb elsewhere; for now, see (*IIJ* 24. 1982: 89ff.).

[10] Older believers in a suffix **-ed-* are listed by Brugmann (1904: 30).

[11] A paradigm he called hysterodynamic and others would call holokinetic.

[12] Beekes is followed, with reservations, by Hamp (*KZ* 96. 1982–1983: 101). But Hamp does not address the weaknesses raised here.

[13] More material in Brugmann (1906: 468f.); Wackernagel-Debrunner (1954: 174–175). Within Latin, on *cuppes* 'gluttonous' see Leumann (1977: 367, 372); on *custos*, see H. Nowicki (*KZ* 92. 1978) (convincingly < **kudh-to-sd-* 'der bei etwas Verborgenem sitzt'); *merces* 'payment' remains completely obscure.

[14] Roots suggested include 1 **ḱel-* 'warm; cold' (Pokorny (1959: 551): Lat. *caleo, calidus* etc.) or 2 **ḱer(E)-* 'grow' (Pokorny (1959: 577): Lat. *Ceres, creare* etc.).

[15] Again, what is the value of the suffix in **śarad-*?

[16] There is no evidence whatever for a weak stem **ĝheEr-d-*.

[17] Which Frisk disregarded in the rest of his discussion.

[18] Only 10.87.7d (hate-song) *āmádaḥ kṣvíṅkās tám adantv énīḥ* 'Let the raw-eating, *éta- kṣvíṅka*-birds eat him!' (with *figura etymologica).*

[19] Wackernagel (1955: 927). The different color of the contraction is due to the different preceding vowel: **ŏ . . . ŏ* avoided due to labial dissimilation.

[20] Only 1.164.22ab, of *suparṇá-* birds in the tree riddle; Thieme (1949: 65). Johnson (*JAOS* 96. 1976: 248ff.) brings in the "speculative symposium".

[21] Cf. also the group of Latin ornithonyms formed after *fīcĕd-ula*, Leumann (1977: 312). Thieme (1949: 65) argues that RV *madhvád-* means 'fig-eater'.

[22] ὠμηστής refers to οἰωνοί *Il.* 11.454; to κύνες *Il.* 22.67; to ἰχθύες *Il.* 24.82; to ἀνήρ (metaphorically, of Achilles) *Il.* 24.207; to Ἐχιδναν ὄφιν *Theog.* 300; to Κέρβερον κύνα *Theog.* 311.

[23] Normal, non-metaphoric eating and drinking e.g. 14.81; 15.373, 378; 21.69; etc.

[24] With βιότου, cf. ζωὴν in *Theog.* 606, quoted section 2; with κτήματα, cf. κτῆσιν in *Il.* 5.158 (*ibid.*).

[25] Other verbs are used metaphorically of wealth, but not of houses, e.g. δαρδάπτω, κείρω. These too may be poem-specific inventions.

[26] On futuristic *appizziyan*, cf. (*KZ* 96, 1982: 74; 86).

[27] Kronasser (1966: 520); Oettinger (1979: 421). The "Menschenfressertext" (*CTH* 17.1) repeatedly uses *ed-* for cannibalism, not *karep-*. Cf. also Telipinu II. 73, and Goetze 1928: 79–80.

[28] The expression is not totally isolated in Hittite, but parallels are mostly too fragmentary to be of help, e.g. the ritual *KBo* XXIV 93 (*CTH* 670) III 17–8:

. . . *namma ape* É.MEŠ *šer kat*[*ta* . . .

GIŠ*ḫuimbaza karipanzi*. . .

'and these devour the houses from top to bot[tom, from the . . .] and from the *huimba-'* (= floor? roof?). Cf. also line 20 'they devour the wall, beams, windows', again with *karipa*[*nzi.*

[29] Verse *d* is a refrain (= 2*d*, 3*d*), *yás tákṛṇoḥ prathamáṃ sásy ukthyàḥ* 'You who did this first, as such are you praiseworthy'.

[30] *pṛṣṭhá-* means primarily 'back, spine', and secondarily 'ridge, peak'. Oldenberg considered taking the simile as referring literally to a sacrifical meal, but ended up with "Gedeihen verteilend wie Reichtum, der (jedem) zu der Höhe Herbeikommenden zugute kommt". The translation in the text assumes "scheinbar vorangestellt" *iva* in the sense of Oldenberg (1967: 248ff.). Geldner's contorted reading involves arbitrary ellipsis.

[31] Other passages showing *puṣṭí-* = 'wealth': 2.12.5c; 5.10.3.

[32] Cf. also 3.30.7b *ábhaktaṃ cid bhajate gehyàṃ sáḥ*, and 10.112.10d *ábhakte cid á bhajā rāyé asmán.*

[33] "Il est définitivement établi qu' Indra tue en naissant son père Tvaṣṭṛ pour boire le Soma" (Bergaigne 1877: 59).

[34] The standard connection is with **seA-* 'satiate' (Pokorny 1959: 876), thus 'unersättlich'. Note the chance resemblance to σίνομαι 'hurt; despoil', itself usually derived from **twi-n-yo* (Pokorny 1959: 1099) rather than from **si-nw-o-*. **si-* seems not to have been treated in Greek like **su-*, on which cf. *Glotta* 60. 1982: 58–59.

[35] Geldner's interpretation rests on Yāska and other native sources. The riddle of *c* could be thus expressed in American English: 'Who can eat the father's lettuce (= dollars) without using this teeth?' (Answer: a **ĝheEro-Eed-*).

[36] The semantic development 'abandoned' > 'inheritance' seen in **ĝheE-ro-* has two sets of parallels. The first is the oft-cited one of German *Erbe*: Greek ὀρφανός, Latin *orbus*, both the latter meaning 'deprived, bereft; orphan'. The other was pointed out to me by G. Pinault: post-Vedic *rikthábhāj-* 'heir' (Manu+). Besides *bhaj-* = φαγ-, *riktha-* (see fn. 41), like *rékṇas-*, is a term for 'property, inheritance' derived from **leykʷ-* (Pokorny 1959: 669), semantically parallel to **ĝheE-* 'leave'.

[37] More elsewhere. Purūravas vs. Urvaśī is a war of words, not deeds, cf. (*JIES* 7, 1979: 249ff.).

[38] More on animal imagery in Hittite: E. Neu (1974: 103).

[39] The parenthetic phrase *vṛ́ko hí ṣáḥ* at 9.79.3b discussed by Campanile recurs non-parenthetically in 6.51.14d *jahī́ ny àtríṇaṃ paṇíṃ, vṛ́ko hí ṣáḥ* 'Strike down the tightwad Atrin, for he is a wolf'. Cf. also 2.23.7ab *utá vā yó no marcáyād ánāgaso/'rātīvā́ mártaḥ sánukó vṛ́kaḥ*, and 2.28.10c *stenó vā yó dípsati no vṛ́ko vā́*. Avestan *vəhrka-* could provide further material, e.g. *Y.* 9,18.

[40] The χηρωσται' passage *Il.* 5.155–8 also involved merciless slaughter.

[41] Benveniste (1969: 84): "N'étaient dits héritiers que ceux qui héritaient *à défaut* du fils; c'est le cas des χηρωσται' . . ." On exclusion of females, cf. Schrader-Nehring (1917–1929 s.v. Erbschaft), Wackernagel (1920–1924: 28) (*heres* as fem. is late), and *RV* 3.31.2a *ná jāmáye tánvo rikthám áraik*, if the traditional interpretation as 'the direct son has left no inheritance to his sister' is correct.

[42] Could our term be a substantivised epithet for an earlier form now totally lost due to taboo?

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Genetic classification in linguistics and biology

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The man we are honoring by this volume invited me to contribute to a seminar in which biologists and linguists would join in comparing genetic classification in their respective fields. To my great regret I was unable to accept. This article is my contribution to the discussion of this subject.

Linguistic science and biology utilize the same family tree model. In biology the model is used to diagram evolutionary developments leading from an elementary life-form to all of the various members of the contemporary galaxy of life-forms. In linguistic science the same model is used in diagraming the increase in membership in a family of languages. In both fields the family tree diagram is thus a statement of a genetic classification based on hypotheses of origins which are intended to explain data that are observable or otherwise available in the form respectively of fossils and records. We will here try to explore some of the similarities and differences that underlie the use of this model in these fields.

In biology it is commonly agreed to trace all life forms to a single origin. In linguistics it is not common to trace all natural languages to a single common origin. The conclusion that some languages are interrelated – that is, continue the same earlier language – is widely accepted if it can be reached by the linguistic comparative method which demands that certain stringent criteria be met. It seems evident now that this method will not permit the interrelationship of languages if their unity ended before a certain time, say before 15,000-20,000 B.P.; many scholars might put this time much closer to the present. At the same time it is generally accepted that even if the criteria of the comparative method are not met, this fact does not exclude the existence of an interrelationship that the comparative method cannot be used to reach, though, to be sure, such indeterminable interrelationships are presumably always earlier than determinable ones. The comparative method requirements have only a positive effect, not a negative one, except for the range over which they produce positive results.

Although failure to satisfy the requirements of the comparative method is not evidence of unrelatedness, it is common parlance among linguists to speak of languages as being unrelated. This can occur when the comparative method has not given positive results. Thus it is conceivable that languages commonly spoken of as unrelated – instead of, more properly, unrelatable – might nevertheless be related (or interrelated).

In fact there is now good reason to believe that all human (natural) languages have a common source. All varieties of the human species are language-possessing and no other species has what can be said to resemble a human language either in structure or complexity of

semiosis. The simplest hypothesis regarding the differentiation of the human species from its most recently associated relative is that the differentiation was interdependent with the development and exercise of a language capability. It seems hardly likely that this coincidence of speciation with language acquisition could have occurred more than once independently. This proto-language of the species can be aptly called Proto-Human, a term which is not my invention and already has some currency. (I should add that prior to my realization of the need to give weight to the congruence of species with language-possession I was skeptical of the utility of the Proto-Human hypothesis.)

The language type of Proto-Human is perhaps best regarded as being like that of any of the languages that exist today, at least in respect to the factor of equicomplexity. There is a growing tendency to recognize that in some sense all of the world's languages are equal in relation to the task of serving as a means of everyday communication. This recognition can safely be extended to the proposition that any natural language can be brought to the point of serving the same purposes as any other merely by expanding its vocabulary. A possible formulation of this relation between natural languages is that all (human) languages are structurally equicomplex. This theory is justified by the observation that normal individuals become minimally equipped language-manipulators in their respective societies at about the same rate, that is, at about the same age. This observation implies that if we assume that infants show the same average resistance to (or aptitude in) acquiring the structure of the language of their environment, the languages offer about the same degree of difficulty to the infants. This state of affairs can be attributed to the tension between the increasing expertise of older speakers (parents, siblings, and others in the child's environment) and the natural absorptive limitations inherent in the childhood state. Although the power of acquiring a language structure distinguishes the human being, the actual exploitation of this power demands time. It is interesting that an approximate equality in the rate of acquisition is observable among children facing very differently structured languages.

It is hard to believe either (1) that the current universal level of language complexity was reached prior to human speciation and was, as it were, a precondition for human speciation, or (2) that human speciation was a prior condition for the initiation of the first step toward language acquisition. It seems more in keeping with the gradualism that permeates evolutionary evidence to imagine that the very first steps in language development above that which can be observed among the other highest primates occurred in some variety of a primate species which speciated as its means of oral intercommunication advanced, but significantly long before the current level of complexity was reached.

After speciation the evolution toward our current level of language can be regarded has being fostered by the advantages given by their uniquely possessed means of cooperation and of storage and transmission of experience. It is conceivable that as the early language form evolved, dialects developed with the increase of population. The hypothesis that such dialects lie at the basis of our current unrelatable families seems prima facie to be tenable. Under this hypothesis the dialects would have differentiated to the point of being no longer recognizable as the same language; the requisite number of ancestors for our determinable families could be reached either directly from the dialectalization of the original primitive language or by the later branchings of the languages that resulted from the initial subdivision. This hypothesis suffers from the weakness that it either (1) necessitates the assumption that progression toward the current level of complexity is inevitable, since it requires that some number of now different primitive languages reached the same level of

complexity and then stopped complicating, or (2) forces the abandonment of the equicomplexity theory itself, if the level of equicomplexity is not inevitable.

If it be posited on the other hand that there was a single primitive language that progressed to the level of complexity now evidenced by all current languages and that it was the successive branching of this more complex language that ultimately led to our current languages, we have a simpler hypothesis that is equally satisfying.

This reasoning completes the argument for a Proto-Human which was equicomplex with observable languages. At the same time this hypothesis leaves room for the notion that what evolved into Proto-Human was not as complex as current languages. We shall refer to the stages of Pre-Proto-Human after speciation that led to Proto-Human as Primitive Human.

The argument for a Proto-Human presented here should not be regarded as of and by itself justifying the Proto-Human that has been inferred by others from evidentiary arguments based on collections of putative cognate sets. Such sets do not meet the comparative method requirements and there is reason to believe that they can never meet these requirements.

The comparative method requirements for establishing relationship are very strict in the hands of established comparatists. Some might wish to state that a proposed relationship must be shown to be beyond doubt. Since a proposed relationship is a hypothesis by virtue of being a hypothesis of common origin, it cannot be beyond doubt; a hypothesis beyond doubt is a contradiction in terms. If the requirement is relaxed to read 'beyond reasonable doubt', the issue becomes one of calculating probabilities, for the reasonableness of what is reasonably doubled can be increased by the consideration of additional evidence. Unfortunately no satisfactory way of calculating the probabilities involved in a primary relationship hypothesis has been proposed, despite the fact that there is a consensus regarding a large number of primary relationship hypotheses. The strictness of the comparative method requirement seems thus to reside in the fact that any new relationship hypothesis must meet the same requirements as some established (= accepted) hypothesis, for the latter provides the only guidelines. It seems hardly likely that the current extensive and intensive examination of possible but unestablished relationships will not soon eliminate all those which satisfy this requirement, leaving some considerable number of unrelatable language families.

The hypothesis of a Proto-Human does very little to assist in the establishment of the family tree nodes leading to our observable languages. The most that it can do is to enhance the probabilities associated with putative cognate sets on the basis of which claims of new relationships are based. If there was a unified Proto-Human, it is reasonable to believe that the probability that putative cognate sets that look attractive include some really cognate sets is greater than if such a language never existed. At the same time the degree of enhancement is dependent on the estimate of the interval between Proto-Human and the earliest proto-language established through the comparative method. The shorter the interval, the greater the enhancement of the probabilities and, of course, vice versa.

If for the sake of argument the date of Proto-Human is set at 100,000 B.P., the interval approximates 80,000 years or about four to five times the interval over which a relatively optimistic view of the power of the comparative method to establish relationships claims that it operates. It is hardly within the range of reasonable expectation to anticipate that extensive and intensive work on currently established proto-languages might ever bridge

such a gap. The chances are obviously better if the dissolution of Proto-Human is set at a more recent date; the collection of cognate sets would then appear to have better chances of containing some truly cognate sets. However it also appears inevitably true that the closer the date of Proto-Human is set to the present, the greater the difficulty faced by the hypothesis is in explaining the extreme divergences among the world's language families.

It is intuitively satisfying to regard the child's acquisition of language as roughly recapitulating the evolution of language through primitive stages to the present equilibrial state that we have termed equicomplexity. There can hardly be any doubt that the phonemic principle distinguishes human languages from animal intercommunication and characterized the initial steps above the latter from the very start. This principle can be seen as permitting the expansion – gradual or explosive – of grammatical structures to reach the plateau represented by equicomplexity. Thus it would appear to follow that the child's acquisition of the phonemic principle before the acquisition of grammatical intricacies resembles such a recapitulation. Another example, trivial though it may be, is the acquisition of words before word combinations. Other examples of recapitulations are bound to be discovered as the typological (i.e. cross-linguistic) analysis of the child's acquisition of language advances.

The time between the acquisition of the phonemic principle by the Pre-Proto-Human and the attainment of the highest complexity plateau (= the level of equicomplexity) by the Primitive Human must have been relatively short, for if it were long, the population could be expected to have increased to the point of necessitating separations that might result in at least markedly different dialects if not different languages. It is difficult to believe that the entropy of the intercommunicational system characterizing all of the languages that we know would not have begun to operate immediately. The rate at which the homogeneity of idiolects in languages is dissipated depends on the rate of intercommunication between contemporaries. As this rate falls toward zero, the rate of divergence increases, and at zero the rate of divergence is maximum. Thus the longer the duration of Primitive Human, the farther its dialectalization would have proceeded toward the outcome of different languages. Although this outcome can not be ruled out, the hypothesis of a unified Proto-Human appears to require that the time for Primitive Human be both significantly long and relatively short, say, at most a thousand to fifteen hundred years.

It seems both convenient and reasonable, though not at all satisfying, to regard the child as having acquired a language at the moment that he has acquired a phonemic system appropriate to that language. The child probably acquires the phonemic principle during his second year and attains a complete phonemic system during his fourth year or shortly afterward. Roughly speaking then, the 'gestation' period of a language takes about two years, followed by years of increasing expertise and decreasing rate of advance. From this point of view, the time needed for a child's acquisition of a language – viewed as a recapitulation – is proportionately a larger percentage of the duration of Primitive Human than the percentage that human gestation – viewed as a fetus's recapitulation of human evolution – is of the time over which human evolution took place.

Although without the phonemic principle grammatical complexities were probably unachievable originally, it does not follow that the child must acquire the complete phonemic system of his language before he acquires any of the grammatical structure. Rather one can expect the notion of arbitrarily different sounds as distinctions to appear before gram-

matical features are acquired. The child learns to deal with words, or what are effectively words, before he learns ways of combining them. The distinction of grammatical features can thus be regarded as in general dependent on the prior acquisition of a phonemic system of some size, minimal though it may be. Nevertheless we can also expect some grammatical features to appear before the child has completely attained his version of the phonemic system of his language.

Within our present framework, as the theoretical moment of completing the acquisition of a phonemic system approaches, the semasiological equipment of the child, on the average, is in some sense about the same for all languages. This implies that their vocabularies are about equivalent. It may very well be that the corresponding equipment of adults differs in societies differing in development; this is indeed likely to be the case in view of the aids and requirements that developed societies present to their members. However since children, if unhampered by the debilitating effects of disease, malnutrition, and neglect (theoretically irrelevant and thus discountable despite their practical effects on an average) can be expected to pursue the acquisition of language competence at about the same rate, their semasiological complement should also advance at about the same rate. The apparent advantage offered by languages with smaller phonemic inventories, which might appear to permit the semasiological component to increase more rapidly, is offset if – as we have some reason to believe – smaller phonemic inventories require longer meaningful units.

The phonemic principle lies at the very heart of the comparative method. The comparative method is based on the law of regular phonetic change which requires that in continuing words a phoneme can change in different ways only in differing phonemic environments. Associated with this law is the law of dialect cohesion which requires that all pairs of idiolects (or dialects) of the same language should show large numbers of approximately homosemantic words whose phonemes correspond systematically and are most heavily distributed among the words of highest frequency in both members of the pair. The consequences of the law are that dialects of the same language universally exhibit large numbers of words with the same or similar meaning whose phonemes correspond systematically, a relation which can be regarded as facilitating the intercommunication between speakers of phonemically differing dialects.

Extra-legal changes involving phonemes are in general sporadic and can nearly all be classified as analogical changes or spurious because they involve borrowing. Since borrowing occurs not only between different languages, but also between different dialects of the same language, a given dialect may show sets of doublets that differ regularly with each other.

The equicomplexity of language structures does not appear to have an analog in outcomes of biological evolution. One might look for it in local adaptation to the environment, for each species almost universally seems admirably adapted for survival in its environment and must be so on pain of extinction. Nevertheless unicellular animals are inescapably less complex than multicellular animals. Furthermore the differences in complexity of brain structure found in higher animals can hardly be equated with differences in structural complexities elsewhere in lower organisms.

The dialectological language appears to be a nice analog of a biological bisexual species. A dialectological language consists of all idiolects (or, it one wishes, dialects) connected by a chain of mutually intelligible pairs of native speakers. Similarly a bisexual species can be

defined as consisting of all varieties (or individuals) connected by a chain of pairs of viably reproductive individuals (in the sense of being able to produce fertile offspring). A chain of pairs as used here is a set of all pairs (1) arranged so that each individual is a member of as many pairs as there are other individuals with whom it shares the defining criterion and (2) connected together by an imaginary line (or curve) which can pass through all individuals regardless of geographical distribution by passing from one pair to another only through a common member, retracing being permitted.

Such an arrangement seems to suggest the possibility that, within the set of individuals who can interact, a minimum rate of change is operative so that though local homogeneity is maintained, diversity develops among non-interacting individuals. When the individuals of different groups interact in reduced degree or not at all, the changes affecting their respective dialects tend to be independent. It is the rate of interaction – in the case of languages, intercommunication – that determines both homogeneity and diversity within a language, but only because of the constancy of change. The fact that languages behave according to this model is exemplified by all languages whose history can be traced through records. All have changed in some degree even when the elapsed time is only a few hundred years. At the same time, obviously, no difficulty in intercommunication arose locally, whereas among groups who for some reason were separated, great differences have arisen, sometimes, as in the case of Rumanian which became separated from the rest of the dialects that continued Latin, leading to the rise of different languages. In this theory of a minimum rate of change there is at least a partial explanation of the fact that all languages exhibit dialectalization.

This model does not fit evolutionary developments in biological evolution so well if it is true that some species have been able to persist essentially unchanged over extremely long periods of time. A fossilized ant from a period in the far distant past is reported to be indistinguishable from a contemporary species. At the same time it could be true that there is a difference in this respect according to position on the evolutionary scale, so that the higher species are subject to a minimum rate of change, whereas lower species are not. Perhaps it is also of some importance that asexual species have no analog among languages.

The manner of language increase and that of species appears nevertheless to be essentially the same. Both result from the increasing divergence between unispecial non-interacting varieties. A final split can not occur between interacting varieties. The final split can thus occur only (1) if parts of a chain become isolated from each other so that no interaction takes place between any members of the separated parts for the length of time necessary for changes on both sides to destroy the possibility of interaction or (2) if a linking group between extremely divergent parts of a highly diversified chain is wiped out so as to leave only groups that can not interact with each other.

Such an increase has a superficial resemblance to mitosis since what was a unified whole becomes separate units. Although linguists appear to permit splitting simultaneously into more than two branches at once, they do so when – as happens in many instances – their evidence does not allow distinguishing the sequence of some number of divisions. This might result from splits which occur too close together in time to give sufficient evidence to determine their sequence securely. Such supraunitary splits are permitted in the biological realm for the same reason.

In linguistics the dominant trend among comparatists is to insist on the non-merging principle. They assume that languages do not mix, at least in the sense that a language boundary, viewed as what occurs between dialectological chains regardless of geographical distribution, can not disappear except with the demise of one of the bounded languages. This principle can only be maintained under a definition of 'a language' that is essentially like the one presented above for a dialectological language and if it is also recognized that a creole language like Takitaki (qua dialectological language, not to be confused with a creole dialect of some dialectological language like Jamaican English) is not a-native-speaker-to-native-speaker continuation of any other languages or language, for that matter, and is thus a newly originated language. The native idiolects associated with it from the time it acquired native speakers were never part of any other chain. In this way, though a creole language may satisfy the notion of mixture under some definition of mixture, it does not exemplify language merger in any sense that affects the traditional notion that a language boundary that has once appeared cannot disappear through such an event.

The non-merging principle fits with our intuition as to what can be expected to happen if people speaking different languages begin to interact. Normally an agreement can be expected to be reached as to which language will be used with each other by members of different groups. Though loanwords from the other language can be expected to be brought into the agreed-upon language, the integrity of the latter would not be affected. Creole languages are best regarded as a product of the special conditions that arose during the post-Columbian colonial period since there is no indication that creole languages were produced in slave-collecting empires before that time.

The importance of the non-merging principle for making inferences about the past of languages is that it serves to simplify the task of establishing and reconstructing proto-languages. As a result of the operation of the law of regular phonetic change, cognates (i.e. words that continue the same word from the common proto-language) show the same sort of systematic correspondences that are shown by so many words of the same or similar meaning in different dialects of the same language. To be sure the phonemic correspondences between words of different languages involve differences of greater complexity than those shown by words of different dialects of the same language. Nevertheless it is just such systematic correspondences that permit not only the identification of related languages, but also directly the reconstruction of the phonology of the proto-language continued by the related languages – each in its own way – and indirectly many of the other features of the proto-language, such as at least in part, where not fully, its structure in the form of its morphology and syntax as well as even the meaning of some of the words that can be reconstructed. If languages could merge, such inferences would be difficult if not impossible.

It is perhaps worth observing at this point that the non-merging principle applies to what is expected to occur in nature. One can imagine that with a great deal of effort a connection over gradually varying dialects could be induced artificially, just as, I believe, it is possible to manipulate the genes to produce interspecial varieties which would then constitute a connection between what had been before regarded as different species. Our definitions apparently cannot be expected to operate in the face of artificial interference.

I am not familiar enough with the course of reconstruction from the comparison of occurrent forms in biology to do better than to comment that it is my general impression

that the reconstruction of a proto-creature through this type of inference is not generally engaged in. There is no clear analog of the law of regular phonetic change, but there is almost obviously strong reason to hope that the necessary information will prove attainable from the triads of the helixes to ultimately provide the steps by which the characteristics of 'missing links' can be inferred and even the chronological periods of their existence.

Some notes on Dravidian intensives*

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Reduplication of forms in the Dravidian languages has not yet received full treatment, either descriptive or comparative-historical. Nor does it seem probable that the latter will be possible in any productive sense until full descriptive treatments of at least a majority of the languages are available.

It is already evident, however, that several different semantic categories are involved in this type of morphology, two important ones being distribution ('street after street', 'ten each', 'each by itself') and intensity ('high degree', 'excellence', 'emphasis', 'very'). A third category which involves reduplication consists of expressives (onomatopoetics). In this latter category it has already been shown that the morphology sometimes has instead of identical reduplication some substitution for part of the basic form (e.g. Ta. *kaṭapaṭāv-eṉal* 'hullabaloo') (Emeneau 1969: 274–299). A semantic category which is always expressed by reduplication with substitution is that of the 'echo-words' ('A and the like', 'A etc.'; the formation: C$\breve{\text{V}}$X g$\breve{\text{i}}$X) (Emeneau 1938: 109–117).

Pending the desirable full treatment, which is probably realizable only in the distant future, this paper calls attention to some details of the intensive formation.[1] No full treatment is yet at hand for any of the languages. The dictionaries are hardly exhaustive, and the etymological dictionary (DED, etc.) did not succeed in recording everything that was available. Kittel's description of the formation in Kannaḍa is perhaps as good as any, with numerous examples (from the literature) divided by 'parts of speech'; e.g. *huḷi huḷi* 'very sour (of buttermilk)', *heccu heccige* 'more and more, excessively', *beḷa beḷagu* 'excessive lustre', *piri piridu* 'that is very large', *kappa kappane* 'very black'. The *Tamil Lexicon* contains many examples; e.g. *veḷuveḷu* 'to become white, pale', *veḷu-veḷu-eṉal* 'extreme whiteness or paleness', *veḷuveḷuppu* 'whiteness, pallor' (: *veḷ* 'white', *veḷu* 'to become white, pale'); *kaṟukaṟu* 'to become very black' (: *kaṟu* 'to grow black'); *tuṭituṭi* 'to be in a great flurry, fret and fume' (: *tuṭi* 'to be in great flurry or agitation, tremble, palpitate'); *cilircilir* 'to get goose-skin from intense emotion' (: *cilir* 'to bristle as the hair on the body'); *kuṟaikuṟai* 'to be confused' (: *kuṟai* 'to be troubled'); *karakara* 'to feel irritation (in eye, throat)' (: *kari id.*). Telugu has the formation; e.g. *ceṟaceṟal-āḍu* 'to be angry or furious'; *koṟakoṟa* 'anger; angrily', *koṟakoṟal-āḍu* 'to be angry, have an angry look'. Examples are found in the descriptions of the non-literary languages of the center and the north, but there

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* In this volume honoring Henry Hoenigswald on his 70th birthday, I add my homage to one who has notably advanced comparative-historical linguistic methodology. My paper illustrates how abortive such a study may be when we lack adequate descriptive data, but also, I hope, that to try to point out a direction for further research may promise future profit.

is no satisfactory description, and such an example as Kur. *bilbilrnā* 'to sparkle, twinkle' (: *bilcnā* 'to shine, glitter, sparkle'), Malt. *bilbilre* 'to shine brilliantly', alongside Ta. *veḷuveḷu*, etc. (quoted above), Ma. *veḷuveḷa* 'very white' (: *veḷu-* 'to be white'), can hardly be evaluated.

Kittel defined the intensive reduplication in Kannaḍa as meaning: "to express high degree, excellence, intensity or emphasis" (1903: 302ff., §§ 303.1(d), 2(a), 305(a), 307(a)), also using 'very' commonly in his translations of examples.[2] Arden (1934: 283, § 579(v)) on Tamil says: "the first part is reduplicated to intensify the meaning, as *naṭunaṭuṅku tremble greatly*, a reduplicated form of *naṭuṅku tremble*". Such forms in the *Tamil Lexicon* are sometimes translated with an indication of this meaning, sometimes not. Of the several forms connected with *veḷu* 'to become white, pale' that were quoted above, *veḷuveḷu* is given with the same meaning as *veḷu*, but *veḷu-veḷuv-eṉal* is defined as 'extreme whiteness or paleness'. Of the other forms quoted above, *kaṟukaṟu* 'to become very black' is so defined as contrasted with *kaṟu* 'to grow black'; but for none of the other reduplicated forms is there given a meaning that explicitly includes 'intensity'. Similarly with most of the other reduplicated forms that could be quoted; the simplex form and the reduplicated form are usually given identical or almost identical meanings, and we must conclude, by extrapolation from the few examples in which the explicit Kannaḍa meaning of 'intensity' is paralleled, that the compilers of the *Tamil Lexicon* have been somewhat inexact in the meanings they give.

Once the intensive formation is established, it is possible to use it as the basis for some formations whose meaning is 'maximal intensity'. The difficulty caused by incomplete recording of forms and defective dictionary meanings is particularly evident here. Very few contrasting pairs of forms are available in the material at hand. Entry 1 below, Ta. *puttapputiya, puttamputiya* 'brand-new, very recent' (: *putu, putiya* 'new'), establishes the meaning; similar are entry 4 Ta. *verra-veritu* 'absolute worthlessness', entry 12 Ta. *tannan-taṉi* 'quite alone', entry 7 Ka. *kaṭṭakaḍe*, Te. *kaṭṭakaḍa* 'the very end'; for none of these is there recorded an intensive formation (**veṟaveṟitu*, etc.). A few examples have contrasting pairs, but in no case is there recorded contrasting meaning; e.g. entry 14 Ta. *veḷuveḷu* 'to become white, pale', *veḷu-veḷuv-eṉal* 'extreme whiteness or paleness': *veḷḷa-viḷar* 'to become very white (as a washed cloth)' (: *viḷar* 'to become pale, whiten' = *veḷu*); see also entries 3, 5, 15, 16, 17.

Clear examples yield the following statement of formation: in C^1VC^2V of the initial member of the reduplicated form, C^2 being a coronal (Schiffman 1975: 69–85; 1980: 101–110) consonant (but not *c*),

voiced stops *d ṟ ḍ* → voiceless stops *tt rr ṭṭ* (in Tamil-Malayalam the voiced stops are written *t ṟ ṭ*);
nasals *n ṇ* → *nn ṇṇ*;
lateral *l* → *ll*.

The largest number of examples has been recorded for Tamil. There are examples also for Kannaḍa, Telugu, and Kota. Some etymological groupings are found in the material, but the almost complete certainty that the material at hand is only a sample makes it impossible to attempt comparative-historical statements which would settle such questions as whether we can reconstruct the formation for South Dravidian (or general Dravidian?), or whether there has been extensive borrowing from one language of origin (e.g. Tamil).

The entries for the 'maximal intensive':

**d → *tt*

(1) Ta. *puttapputiya, puttamputiya* 'brand-new, very recent' (: *putu, putiya* 'new') (3511/4275)

(2) Ka. *tuttatudi* 'the very point or end' (: *tudi* 'end'; cf. Ta. *tuti* 'point', Te. *tuda, tudi* 'end') (2716/3314)

(3) Ka. *motta-modalu* 'the very beginning, at the very first' (: *moda-modalu, modalu modalu* id., *modal(u)* 'state of being first') (4053/4950)

**ṟ → *ṟṟ*

(4) Ta. *verṟa-veṟitu* 'absolute worthlessness' (: *veṟitu* 'uselessness, futility', *veṟu* 'empty, useless, etc.'), Ko. *veṭa· ver* 'mere, unmixed, vain' (: *ver* id.) (4538/5513)

**ḍ → *ṭṭ*

(5) Ta. *naṭṭa-naṭu* 'the very middle' (: *naṭu* 'middle'), Ka. *naṭṭa-naḍuve* 'in the very middle' (: *naḍu-naḍuve id., naḍu* 'middle', adj. *naṭṭa*), Te. *naṭṭa-naḍumu* 'the very middle' (: *naḍu* 'middle', adj. *naṭṭ-*), Ko. *naṭa· naṛv* 'very center' (*naṛv* 'middle, center') (2959/3584)

(6) Ta. *neṭṭa-neṭumai* 'great length, excessive tallness or height' (: *neṭu* 'long; to be long, tall', *neṭumai* 'length, tallness, height', *neṭṭam, neṭṭu* id.) (3099/3738)

(7) Ka. *kaṭṭakaḍe* 'the very end; at the very end, at last' (: *kaḍe* 'end, last'), Te. *kaṭṭakaḍa* 'the very end, the very last place or point; farthest, hindmost, the very last' (: *kaḍa* 'end, extremity') (929/1109)

(8) Ko. *ceṭ ceyṛ* 'bad smell' (: *ceyṛ, ceṭ* id.; cf. Ta. *ceṭi* 'stench', Tulu *seḍi* 'strong smelling', *seṭṭuni* 'to decay') (1614/2760)

(9) Ko. *muṭa· muṛmuṛṇ* 'without any reason' (: *muṛmuṛṇ* id.; cf. Koḍagu *muṇḍate* 'for no reason') (4037/4928)

(10)? Ta. *veṭṭaviṭi* 'break of day, early dawn' (: *viṭi* 'dawn; to dawn', *veṭṭam* 'light', *veṭṭa* 'clear, plain'; cf. *veṭṭa-veḷi* 'open plain', *veṭṭa-veḷiccam* 'broad daylight'). Perhaps a compound with *veṭṭa*. (4504/5475)

**n → *nn*

(11) Ta. *inṉ-iṉi* 'now, without a moment's delay' (: *iṉi* 'now') (351/410)

(12) Ta. *taṉṉan-taṉi* 'quite alone' (: *taṉi* 'solitude; to be alone') (2612/3196)

**ṇ → *ṇṇ*

(13) Ta. *aṇṇaṇi* 'in close proximity' (: *aṇi* 'near') (69/120)

**ḷ → *ḷḷ*

(14) Ta. *veḷḷa-viḷar* 'to become very white (as a washed cloth). (: *veḷuveḷu* 'to become white, pale', *veḷu-veḷuv-eṉal* 'extreme whiteness or paleness', *viḷar* 'to become pale, whiten', *veḷ* 'white', *veḷu* 'to become white or pale'; cf. Ma. *veḷuveḷa* 'very white', Kur. *belbelrnā* 'to have an unusual or morbid whiteness of the skin, be an albino', *bilcnā* 'to shine, glitter, sparkle', *bilbilrnā* 'to sparkle, twinkle', Malt. *bilbilre* 'to shine brilliantly') (4524/5496)

(15) Ta. *kuḷḷa-kkuḷir* 'to be intensely cool and refreshing' (: *kuḷir* 'to be cool, refreshing'; cf. Ma. *kuḷukuḷu* 'intense cold', Ko. *kuḷkuḷ in-* 'to feel cool, feel calm and peaceful') (1523/1834)

Two other sets of forms may belong here, with rules

**r → *nn* and **l → *nn*

(16) Ta. *pennam-periya, pennam-perutta* 'very large' (: *periya, perutta* 'large', [Caldwell quoted above] *periya periya* 'very great'; cf. Te. *penu* [before consonant], *penn-* [before vowel] 'large') (3613/4411)

(17) Ta. *punnam-pulari* 'early dawn' (: *pulari* 'dawn', *pular* 'to dawn', *puṟ-pull-eṇal* 'break of day'; cf. Malt. *pulpulre* 'to shine through whiteness') (3531/4305)

No instances have been found involving non-coronal consonants (labial, velar), nor the palatal *c*. The following unique set of forms is very puzzling, but may possibly belong here.

(18) Ta. *cekka-cciva, cekkañ-ceku* 'to be deep red', *cekka-ccivēr-eṇal, cekka-ccevēr-eṇal* 'deep redness' (: *civa* 'to redden', *civēr-eṇal, cevēr-eṇal, cekkam* 'redness', but no **ceku!*) (1607/1931)

Two sets in Kannaḍa and Telugu have items in which

**d → *ṭṭ*, instead of **d → *tt*. In one set Kannaḍa has both forms, the expected **tt* having been given above as entry 2. Nothing further of this kind has been found.

(19) Ka. *tuṭṭatudi* 'the very end or point' (cf. *tuttatudi* in entry 2, Te. *tuṭṭatuda* 'the very end or extremity' (2716/3314)

(20) Te. *moṭṭa-modalu* 'the very beginning', *moṭṭa-modaṭa(n)* 'at the very beginning' (: *modalu* 'beginning', *modaṭa(n)* 'at first, in the beginning'; cf. Ka. *motta-modalu* in entry 3 (4053/4950)

Several formations which show some similarities to those already treated are to be analyzed as compounds. Entries 21 to 23 have nothing to do with 19 and 20 in spite of *ṭṭ* in the first elements.

(21) Ta. *kaṭṭu-kkāval* 'strict guard', Ko. *kaṭ-ka·vl* 'close watch'. The first element is Ta. *kaṭṭu* 'firmness, strength' (962/1148); *kāval* 'guard, watching' (1192/1416). Kota has undoubtedly borrowed from Tamil.

(22) Ka. *koṭṭa-kone* 'extreme point', Te. *koṭṭa-kona* 'the very end or extremity'. **koṭṭu* 'tip, point, crest' (1704/2049); **konay* 'tip, point, extremity' (1807/2174).

(23) Ta. *paṭṭa-ppakal*, Ma. *paṭṭā-pakal*, Te. *paṭṭa-pagalu* 'broad daylight, midday', To. *oṭ oxol* 'broad daylight' (used in song). **pakal* 'day(time)' (3151/3805); with **paṭṭa-*, cf. Ko. *paṭa·r in-* '(country) becomes fully light at dawn'. The Toda form has the earmarks of a borrowing from Kannaḍa (Badaga dialect), i.e. **p-* > Ka. *h-* > To. *ϕ*, but the source form has not been recorded so far.

(24) Ta. *cinnañ-ciṟiya* 'very small'. The *Tamil Lexicon* calls this reduplication, but it is rather a compound of *cinna* (2135/2594) and *ciṟiya* (1326/1594), both 'small'; the compound is not in the etymological dictionary.

(25) Te. *niṟṟa-nīlugu* 'to be very haughty'. Te. *nīlugu* 'to be conceited' (3059/3692); for *niṟṟa-*, cf. *niṟu* 'very, much' (3049/3682).

Notes

[1] Most of the data in the paper come from Burrow-Emeneau (1961), *A Dravidian Etymological Dictionary* (DED), its *Supplement* (DEDS), and the 2nd edition revised (DEDR). A few items have been added from other sources, especially Kannaḍa items from Kittel's grammar and dictionary. The paper would have been bettered by consultation with speakers of the languages, but this could not have been easily accomplished. – Each of the numbered entries has at the end a pair of numbers, the first a reference to DED and DEDS, the second to DEDR. – Abbreviations of language names: Ta. = Tamil, Ma. = Malayalam, Ko. = Kota, To. = Toda, Ka. = Kannaḍa, Te. = Telugu, Kur. = Kuṛux, Malt. = Malto.

[2] Caldwell earlier had mentioned the construction in his treatment of the superlative of adjectives, as 'the very primitive plan of doubling of the adjective itself – e.g. [Ta.] *periya-periya*, very great' (1913: 317).

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Vedisch *ā́yavasa-*

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§ 1. Ein Nominalstamm *ā́yavasa-* ist zuerst im Ṛgveda überliefert. Er kommt hier nur an einer Stelle vor, I 122,15. Der schwierige erste Halbvers lautet:

catvā́ro mā maśarśā́rasya śíśvas
tráyo rā́jña ā́yavasasya jiṣṇóḥ.

In Graßmanns RV-Übersetzung findet sich folgende vertretbare Wiedergabe: 'Mich [fahren] vier vom Sohne des Maçarçāra geschenkte Rosse und drei des siegreichen Königs Ajavasa'. Geldners Übertragung weicht teilweise ab; sie ist deswegen einstweilen wohl nicht vorzuziehen, weil er in *śíśvaḥ* nicht wie Graßmann eine Genitiv-Singular-, sondern eine ungewöhnliche – wenn auch nicht undenkbare – Nominativ-Plural-Form sieht. Über *ā́yavasasya* jedoch hat Geldner dieselbe Meinung wie Graßmann: Auch er nimmt hier einen Personennamen im Genitiv Singular an[1]. Diese Bestimmung dürfte richtig sein; sie wird auch von anderen Forschern vertreten und findet sich bereits bei Sāyaṇa.

Falls der Name *Ā́yavasa-* mit dem späteren homonymen Appellativum eins ist, so hat er ursprünglich etwa 'keine Weide habend, weidelos' bedeutet (§ 2). Möglich ist ein Personenname mit dieser Bedeutung zweifellos. Als Stütze für diese Annahme kann – neben der Form *ā́yavasa-* selbst – das vṛddhierte Patronymikon *Sauyavasi-* (AB) angeführt werden. Es dürfte nämlich zeigen, daß auch das Grundwort *sŭ́yavasa-* (§§ 4–5), das Gegenstück von *ā́yavasa-*, als Personenname dienen konnte ('mit guter Weide'). Doch muß eingeräumt werden, daß Bildung und Bedeutung von RV *ā́yavasa-* nicht ganz sicher zu bestimmen sind. Trotzdem ist der vereinzelte Beleg eine wichtige Stütze für die ohnehin wahrscheinliche Annahme, daß das erst etwas später bezeugte appellativische *ā́yavasa-* in ältere Zeit zurückgeht.

§ 2. Dieses appellativische *ā́yavasa-* ist unter anderem in der Taittirīya-Saṃhitā (TS) überliefert, V 2,8,3. Hier wird begründet, warum beim Bau des Feueraltars auch eine *dūrveṣṭakā́-*, ein 'Ziegel mit *dū́rvā-*Gras, Dūrvā-Ziegel'[2], hinzulegen ist:

paśúḥ / vā́ eṣá yád agnír, ná khálu vái paśáva ā́yavase ramante, dūrveṣṭakā́m úpa dadhāti, paśū́nāṃ dhṛ́tyai

'Ein Tier ist ja Agni; Tiere verweilen ja bekanntlich *ā́yavase* nicht; man legt einen Dūrvā-Ziegel hin; das geschieht zur Bewahrung der Tiere'.

Der mit einem Tier identifizierte Agni wird sich also, so erwartet der Opfernde, bei dem Dūrvā-Gras behaglich fühlen[3]. Begründet wird diese Erwartung mit der in dem Satz *ná*

khálu vái paśáva ā́yavase ramante ausgedrückten Lebenserfahrung. Da er die Satznegation *ná* enthält, muß substantivisches *ā́yavasa-* hier eine Gegebenheit bezeichnen, die zwar etwas mit *yávasa-* Ntr. 'Weide' zu tun hat, aber bei Tieren kein behagliches Gefühl bewirkt[4]. Der Kommentar übersetzt *ā́yavase* ansprechend mit *alpaghāse pradeśe* 'an einem Ort mit wenig Nahrung'. Nur geringfügig anders lauten die Wiedergaben von Böhtlingk (Sanskrit-Wörterbuch)[5] und von Keith (TS-Übersetzung): 'Futtermangel' bzw. 'want of grass'. Diese Auffassung ist ebenfalls möglich; da aber bei *ram* häufig Ortsangaben erscheinen, ist 'weideloser Ort, futtergrasloser Ort' vielleicht vorzuziehen.

§ 3. Die TS-Stelle hat in den anderen Saṃhitās des Schwarzen Yajurveda Parallelen. Dabei treten gewisse Abweichungen in Erscheinung; der Satz mit *ā́yavase* jedoch lautet, von Partikeln abgesehen, überall gleich. MS III 2,6: 25,1 *paśávo vā́ iṣṭakā, ná vái paśávā ā́yavase ramante, yád dūrveṣṭakā́m upadádhāty āyátanam iva vā́ etát kriyate, paśū́nā́ṃ yáty(ai)* 'Tiere sind ja die Ziegel, Tiere verweilen an einem Ort ohne Weide nicht; dadurch, daß er einen Dūrvā-Ziegel hinlegt, wird ja gleichsam ein Standort gemacht; das geschieht zum Festhalten der Tiere'. – KapKS XXXI 8 : 180, 23 = KS XX 6 : 24, 19 *athaiṣā dūrveṣṭakā/paśur vā agniḥ/na paśava āyavase ramante/paśubhya evaitad āyatanaṃ karoti/paśūnāṃ dhṛ̥tyai* 'nun der Dūrvā-Ziegel; ein Tier ist ja Agni; nicht verweilen Tiere an einem Ort ohne Weide; so macht er den Tieren einen Standort; das geschieht zur Bewahrung der Tiere'. – Aus einer weiteren Parallelstelle könnte schließlich das Zitat[6] bei Patañjali V 4, 36, 6 (II p. 435, 22 Kielhorn) stammen: *āyavase vardhante* '(sie) gedeihen an einem Ort ohne Weide'. Vielleicht hat der Satz an der verloren gegangenen Stelle vollständig etwa so gelautet: *na vai paśava āyavase vardhante**[7]. Falls diese Vermutung zutreffen sollte, könnten alle bisher bekannten Belege von ved. *ā́yavasa-* ein und demselben Sprichwort[8] zugerechnet werden.

§ 4. Die negative Bedeutungskomponente von *ā́yavasa-* deutet darauf hin, daß in dem *ā́-* irgendwie das Privativpräfix enthalten ist. Wenn dessen übliche Form mit kurzem *a-* angenommen werden soll, so müßte das *ā́* wohl entweder durch Vr̥ddhi oder durch die Lokalpartikel *ā́* zustandegekommen sein. Folgende zwei Wege kämen dann etwa in Frage: 1) *yavasa-* → *a-yavasa-* → *āyavasa-* (mit Vr̥ddhi); 2) *yavasa-* → *a-yavasa-* → (*ā-* 'in, bei' + *ayavasa-* >) *āyavasa-*.

Vr̥ddhi von Privativbildungen (gemäß 1) kommt schon im RV vor (*āmitrá-* : *amítra-*); die Annahme einer solchen Bildung könnte insbesondere durch das parallele, nur wenig jüngere und tatsächlich vr̥ddhierte *sauyavasa-* abgestützt werden, das sogar in einem ganz ähnlichen Wortlaut vorkommt, KB XI 5 am Ende (bisher die einzige Stelle): *upanivartam iva vai paśavaḥ sauyavase ramante* 'irgendwie immer wiederkehrend verweilen ja Tiere in einer Gegend mit guten Weiden'. – Auch ein Kompositum mit *ā́* 'in, bei' als Vorderglied (gemäß 2) wäre immerhin denkbar, wenn diese Bildeweise in der älteren Sprache auch nicht sehr häufig zu sein scheint[9].

Beide Deutungen haben indes einen gemeinsamen Mangel. Sie setzen als Brücke zwischen *yávasa-* und *ā́yavasa-* eine einfache Privativbildung *a-yavasa-* voraus. Es ist nicht recht einzusehen, warum sie trotz ihrer Einfachheit nicht literarisch überliefert ist[10]. Dabei kann eine solche Privativbildung angesichts der guten Bezeugung des einfachen *sŭ-yavasa-* (RV +; der Akzent schwankt) durchaus erwartet werden. Ist doch *sŭ-yavasa-* mit seiner Bedeutung ('mit guter Weide', 'gute Weide') etwas wie ein Gegenstück zu *ā́yavasa-* und enthält sicher nur die zwei Elemente *sú* 'gut' und *yávasa-* 'Weide'.

§ 5. Gerade *sŭ́yavasa-* mit seinem Wechsel zwischen *sū-* und *su-* eröffnet aber einen anderen Weg zur Erklärung von *á́yavasa-*. Das auffällige *sū-* ist hier eindeutig die ältere Form des Vordergliedes; es gilt im RV für *sūyávasa-* und seine Weiterbildungen[11] durchaus. Der Padapāṭha hat überall *su-* durchgeführt. Auch die einzige Belegstelle in der TS (I 7, 5, 2–3 bis) zeigt im Text das rgvedische *sūyávasa-*, ersetzt durch *su°* im Padapāṭha. Original belegt ist das jüngere *suyavasa-* erst ŚB XI 7,1,1 (bis)[12] und BŚS XXVII 14 : 341,1. Es kann leicht als vereinheitlichte Form erklärt werden; als solche ging es auch in den Padapāṭha ein[13].

§ 6. Weiterhin zeigt sich Länge des Fugenvokals in *kú́-yava-* TS IV 7,4,2, einem Kompositum aus pejorativem *ku-* (ursprünglich 'wo?') und *yáva-* 'Gerste', dem Grundwort von *yávasa-*. Doch ist *kū́yava-* aus zwei Gründen für eine Beweisführung hier weniger gut verwendbar als *sūyávasa-*. Einerseits ist die Form mit Kurzvokal, also *kúyava-* 'Mißernte', früher überliefert (RV); *kū́yava-* erscheint außerhalb der TS[14] nur als Handschriftenvariante in der Parallelstelle MS II 11,4 : 142,1. Andrerseits gibt es neben *ku(-)* auch sonst Formen mit *ū*, z.B. *kū́ cit* 'überall' (RV).

§ 7. Die Fugenvokaldehnung in *sūyávasa-* ist, gemäß einer verbreiteten Anschauung, laryngalbedingt[15]. Gelten dieselben Bedingungen auch für *á́yavasa-*, so enthält dieses als Elemente nur das Privativpräfix und *yávasa-* und ist damit eben das gesuchte einfache Gegenstück (§ 4) zu *sūyávasa-*. Das ist sicher eine morphologisch und lexikalisch befriedigende Erklärung.

§ 8. Zu besprechen bleibt die Länge des *á́-*. Ein brauchbares und schon bekanntes Parallelbeispiel für anlautendes *ā-* < *n̥-ə-* (wie in *jātá-* 'geboren' < **ĝn̥ə₁-to-*) ist *á́sant-* 'nicht seiend, nicht wahr', das im RV fünfmal und nur in alten Büchern deutlich überliefert ist (IV^1 V^1 VII^3). Regelmäßig aussehendes *ásant-* liegt im RV ebenfalls an fünf Stellen deutlich vor, von denen aber drei in einem jungen Buch stehen (VI^1 VII^1 X^3); zwei weitere Belege können wegen des Sandhi *ásant-* oder *á́sant-* enthalten (VII 104, 12b; X 129,1).

Graßmann weist in seinem Wörterbuch darauf hin, daß *á́-* zweimal an Versstellen steht, die eine Kürze begünstigen. Der Padapāṭha hat an sämtlichen Stellen *á-* durchgeführt. In späteren Texten kommen offenbar keine selbständigen Belege für *á́sant-* mehr vor. Bei *á́sant-* hat die weiter abliegende Bedeutung 'unwahr', die bei sicherem *ásant-* im RV fehlt, das Übergewicht[16]. Somit spricht tatsächlich eine ganze Reihe gewichtiger Gründe dafür, daß die Privativbildung *á́sant-* etwas Altertümliches ist. Die Herleitung von *á́sant-* aus **n̥-ə₁sont-* ist Kuryłowicz zu verdanken[17].

§ 9. Die allmähliche Ersetzung von *á́sant-* durch *ásant-*[18] könnte darauf deuten, daß auch in anderen Fällen der Anlaut *ā-* von Privativbildungen einem *a-* Platz gemacht hat. Anwärter für ehemaliges *ā-* sind u.a. vielleicht die im RV bezeugten Stämme *a-dánt-* 'zahnlos', *a-reṇú-* 'staublos', *a-vātá-* 'windlos'. Doch ist stets auch die Möglichkeit in Betracht zu ziehen, daß die Bildung erst erfolgte, als das zweite Element (also *dánt-* 'Zahn' usw.) den anlautenden Laryngal bereits verloren hatte.

§ 10. Erwogen werden muß auch, ob das privative *á́-* von *á́yavasa-* nicht durch sekundäre Ersetzung zustande gekommen ist, z.B. durch Übertragung aus lautgesetzlichen Fällen wie *ásant-*[19]. Eine solche Ersetzung scheint mindestens in einem Fall tatsächlich stattgefunden

zu haben, in *ā́deva-* 'gottlos', das RV II 22,4, VI 49,15, VIII 59,2 (Khila) überliefert ist und die gleiche Bedeutung wie häufiges *ádeva-* hat[20]. Wie das *ā́-* von *ā́deva-* zu erklären ist, ist nicht ohne weiteres ersichtlich. Ausgangspunkt könnte der Vers VI 49,15 sein, in dem kurz nacheinander *ádevīḥ* und *ā́devīḥ* vorkommen. Oldenberg (Noten, zur Stelle) spricht von einem "Streben nach Abwechslung"; derlei mag öfters mitwirken, doch wird es kaum je die einzige Ursache sein[21].

Nun macht das *ā́deva-* der drei RV-Stellen einen eher gekünstelten Eindruck. *áyavasa-* gehört dagegen der Prosa an und wirkt nach Bedeutung und Kontext (§§ 2–3) wie eine Vokabel der Normalsprache. Eine Einwirkung von *ā́sant-* oder von *ā́deva-* läßt sich kaum wahrscheinlich machen; und weitere klare Fälle mit deutlich sekundärem privativem *ā-* wie *ā́deva-* scheint es im Vedischen nicht zu geben. Was sonst dafür beansprucht wurde[22], ist entweder ganz unsicher wie *árupita-* RV IV 5,7 (der Padapāṭha hat hier *á-*), oder es verlangt nach einer anderen Erklärung: *ā́tura-* RV, *ābhú-* RV, *ābhúka-* AV können die Lokalpartikel *ā́* enthalten (Padapāṭha überall *ā-*); *árāti-* ist MS I 5,1 : 67,5 unsichere Variante zu üblichem *árāti-*; *āśauca-* 'Unreinheit' ist aus *áśuci-* unter Einfluß von *śauca-* abgeleitet ("Doppelvr̥ddhi"); überdies ist es spät bezeugt (Sūtra).

§ 11. Das Ergebnis lautet: *áyavasa-* '(Ort) ohne Weide' enthält ein *ā́-*, das aus *n̥* und dem ursprünglich anlautenden Laryngal von *yávasa-* kontrahiert ist. *áyavasa-* muß somit alt sein. Die Beleglage (§§ 1–3) läßt diese Annahme durchaus zu. Wie außerdem avest. *yauuaŋha-* erweist, war das Grundwort mindestens im Urarischen bereits vorhanden[23]. Das *ā́-* von *áyavasa-* wurde deswegen nicht durch *a-* ersetzt, weil die Vokabel nicht mehr wirklich lebendig war[24]. Auch im Padapāṭha des RV und der TS steht *áyavasa-*, nicht *áyavasa-*; diese letztere Form war wohl nicht in Gebrauch.

§ 12. Vielleicht fällt von *áyavasa-* auch Licht auf die vorurgriechische Lautgeschichte. *yáva-* 'Gerste' das Grundwort von *yávasa-*, ist ein Kognat von griechisch *ζειά* 'Getreide'. Es liegt hier eine der bekannten Entsprechungen zwischen einem altindischen *y-* und einem griechischen *ζ-* vor. Da nun das *y-* von altindisch *yáva-* auf *i̯-* zurückgeführt werden muß, findet die Ansicht, daß das *ζ-* von griech. *ζειά* (und dann auch von *ζυγόν* usw.) lautgesetzlich uridg. *i̯-* fortsetzt[25], neue Nahrung. Umgekehrt ist dann wohl anzunehmen, daß der uridg. Anlaut *i̯-* im Griech. durch *h-* vertreten ist[26].

Anmerkungen

1 'Die vier Fohlen des Maśarśāra, die drei des siegreichen Königs Āyavasa (erfreuten) mich'. Ähnlich Renou, Ét. véd. et pāṇ. 5 (1959) p. 7.

2 Zum Sachlichen s. Eggeling zu ŚB VI 2,3,2.

3 Über den besonderen Wert des Dūrvā-Grases Eggeling zu ŚB VI 2,3,2. Vgl. auch AB VIII 8,6 *kṣatraṃ vā etad oṣadhīnāṃ yad dūrvā* 'der Herrscherstand unter den Pflanzen ist das Dūrvā-Gras'.

4 Die Erwähnung von *paśú-* bringt es mit sich, daß die Handlung nicht nur dem Agni, sondern auch den Tieren des Opferers zugute kommt (*paśūnāṃ dhr̥tyai*).

5 Böhtlingk/Roth I (1855) hatten noch 'Weideplatz, Futterplatz'; danach Graßmann in seinem Wörterbuch.

6 Wilhelm Rau, Die vedischen Zitate im Vyākaraṇa-Mahābhāṣya (Stuttgart 1985) p. 19. (= Akademie der Wiss. und der Lit., Mainz; Abh. der Geistes- und Sozialwiss. Klasse, Jg. 1985, Nr. 4).

7 Vgl. *paśuvárdhana-* RV 'Gedeihen des Viehs'.

[8] S. noch § 4 (2. Absatz) zu KB XI 5. – Ein verwandter Gedanke z.B. RV V 53,13 *ráṇan gávo ná yávase* 'sie sind vergnügt wie Rinder auf der Weide', ebenso oder ähnlich I 91,13, IV 42,10, VIII 92, 12, X 25,1; *paśúr ná yávase* in Vergleichen V 9,4, VI 2,9, VII 87,2.

[9] Wackernagel/Debrunner, Altind. Gramm. II 1 p. 312, mit Nachtrag.

[10] Nur V. zu Pāṇini V 4,36 (vgl. oben § 3).

[11] *sūyavasād-; sūyavasín-; sūyavasyú-* (mit auffälligem Suffix, vielleicht zu *sūyávasa-* nach einem Paar wie *avasyú-* : *avasá-* gebildet).

[12] Hier auch mit abweichendem Akzent (*suyavasé*). Zur Stelle s. Caland, AcOr 6 (1928) p. 107 (gegen Eggeling, der hier eine besondere Bedeutung annimmt).

[13] Weitere Prosastellen mit dem älteren *sūyavasa-* : KauśS XXIV 17; ĀpŚS VII 28,7; VādhS bei Caland, AcOr 6 (1928) p. 106 (bis). An der zuletzt genannten Stelle ist *sūyavasa-* übrigens maskulines Substantiv; die anderen Belegstellen für 'gute Weide' lassen Maskulinum oder Neutrum zu.

[14] Die Bedeutung von *kúyava-* ist übrigens undeutlich. Es steht in einem Opferspruch, in dem sich der Opferer eine Reihe von – natürlich guten – Dingen wünscht: . . . *pūrṇáṃ ca me pūrṇátaraṃ ca mé, 'kṣitiś ca me kúyavāś ca mé, 'nnaṃ ca mé 'ksuc ca me* . . . 'mir (soll zustande kommen) . . . Volles/Volleres/Nichtversiegen/*kúyavāḥ*/Speise/Nichthungern . . .'. So TS; die Parallelen haben *kūyavam* : MS II 11,4: 142,1; KS XVIII 9: 272,4; KapKS XXVIII 9: 147,6; VS XVIII 10. Da etwas wie 'Mißernte' hier nicht paßt, schlägt Aufrecht bei Böhtlingk (Sanskrit-Wörterbuch V p. 252 b) ansprechend vor, *'kúyavam* (= *akúyavam**) zu lesen und als 'keine Mißernte' zu verstehen. Die (maskuline oder feminine) Pluralform der TS bleibt auch dann noch auffällig; vielleicht liegt hier ein Possessivum vor: '(Jahre/Zeiten o.ä.) ohne Mißernten'.

[15] Diese Auffassung wird seit längerem vertreten; vgl. etwa Cowgill, in: W. Winter (ed.), Evidence for laryngeals (London/Den Haag/Paris 1965) p. 165; Cowgill selbst ist freilich skeptisch.

[16] Oldenberg, Noten zu RV IV 5,14. Hier (und zu V 12,4) auch über Versuche, das *á-* von *ásant-* im Kontext jeweils in *á* + *á-* zu zerlegen. Diese Lösung ist für einen Teil der Belege ganz unwahrscheinlich, naheliegend nirgends; auch der Padapāṭha gibt keine Handhabe.

[17] Symbolae grammaticae in honorem Ioannis Rozwadowski I (Krakau 1927) p. 97 f.; Etudes indo-européennes I (Krakau 1935) p. 30; zuletzt Indogermanische Grammatik II (Heidelberg 1968) p. 201.

[18] Die Ersetzung scheint schon in der Sprache der RV-Dichter begonnen zu haben. Es ist wohl nicht anzunehmen, daß RV *ásant-* (mit *á-*) erst von den Redaktoren des RV stammt. Sie hätten es dann eher überall durchgeführt; zumindest hätten sie kaum in verschiedenen Versen desselben Hymnus (VII 104), ja im selben Pāda (VII 104, 8d) *ásant-* neben *ásant-* geduldet.

[19] So ist griech. *νη-κερδής* (erwartet und jünger bezeugt: *ἀ-κερδής*) nach dem Muster von *νημερτής* gebildet, wo *νη-* < *nā-* alt ist; s. R.S.P. Beekes, The development of the Proto-Indo-European laryngeals in Greek (Den Haag/Paris 1969) p. 101 mit weiteren Beispielen.

[20] Der Padapāṭha hat *ádeva-*, außer VI 49, 15.

[21] Vgl. RV VII 104, 8d (s. oben Anm. 18) mit *ásant-* und *ásant-*; eine Nachahmung gerade dieser Stelle ist aber wohl nicht weiter zu begründen. – Eva Tichy beabsichtigt, *ádeva-* in den MSS zu besprechen; ich verdanke ihr wichtige Hinweise.

[22] S. die Liste bei W. Wüst, Vergleichendes und etymologisches Wörterbuch des Alt-Indoarischen (Heidelberg 1935) p. 208; vgl. auch Bloomfield, Ved. var. II p. 230 f.

[23] Der Wortausgang *-asa-* von urar. **hiauasa-* ist auffällig; kann an ein Kompositum mit der Wurzel **sē* < **seH*1 'säen' gedacht werden, also etwa 'Gersten-Saat'?

[24] Es wäre denkbar, daß im Arischen außer *áyavasa-* noch weitere, möglicherweise stärker verdunkelte alte Privativbildungen unverändert mit *a-* < *n-H-* erhalten geblieben sind, doch ist bisher wohl kein weiterer Fall nachgewiesen oder vorgeschlagen worden.

[25] So zuerst wohl Sapir, Language 14 (1938) p. 271 f.; zustimmend Helmut Rix, Historische Grammatik des Griechischen (Darmstadt 1976) p. 70.

[26] Zu fordern wäre also der Befund: Keine anderweitigen Laryngalspuren bei den Entsprechungen ai. *y-* : griech. *h-* (*yáḥ* : ὅς); doch anderweitige Laryngalspuren bei den Entsprechungen ai. *y-* : griech. *ζ-*. Anderweitige Laryngalspuren liegen tatsächlich vor bei *yugá-* : *ζυγόν*, nämlich u.a. *ayunak* RV, trotz Cowgill (wie oben Anm. 15) p. 164; sowie bei *yáva-* : *ζειά*, nämlich *áyavasa-*, *sūyávasa-* und vielleicht *kúyava-*.

Abkürzungen (Texte, Literatur) im Anschluß an Johanna Narten, Die sigmatischen Aoriste im Veda (Wiesbaden 1964).

Brythonic gender reduction – the Cornish picture

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We have only a vague notion of how genders were distributed in the lexicon of Indo-European, that is, the percentage of masculines, feminines, and neuters – for those are the genders ordinarily assumed for Indo-European. There were probably three genders, at least. Whether grammatical genders exceeded that number we cannot tell, and it is not easy to imagine what additional genders would have been like if they had existed. (When certain non-Indo-European languages are said to have many genders, the term is being used in a different way).

In some groups of languages there has been a reduction of the number of genders. Hittite had common and neuter, and the neuter differed from the common very little in most cases. In Armenian the reduction was drastic and occurred prehistorically, for in all stages of the language known to us there is no grammatical gender (it is hardly more felicitous to say there is one gender, which amounts to none anyway).

In Celtic, as elsewhere, the neuter has been especially vulnerable. Old Irish had masculine, feminine, and neuter gender. But early in the Middle Irish period (probably in the tenth century) the neuter vanished, and nouns formerly neuter became masculine or feminine – more frequently masculine. As Pedersen (1913: 66–67) pointed out, the incipient demise of the neuter can be seen as early as Old Irish, with some originally neuter nouns vacillating between neuter and masculine, and one or two between neuter and feminine. Thurneysen (1946: 154) also calls attention to the partial elimination of gender distinctions in the pronoun, the article, and the adjective long before the noun followed suit.

In Brythonic, according to a belief expressed by several scholars, the neuter was preserved long enough for a number of Latin loanwords of neuter gender to be transmitted by Old British to Old Irish, where they retained their neuter gender. Favorite examples are Old Irish *ór* n. 'gold', from a British equivalent of Welsh *aur* (now masculine, like Modern Irish *ór*) and Old Irish *fín* n. 'wine' (now masculine, like Welsh *gwin*, from Latin *vīnum* n.) (Pedersen 1913: 66). I am not persuaded that this guarantees that Old British still had a neuter; there could have been re-assignment of the words to the neuter gender in Irish for some reason now obscure. The British Latin words themselves could even have lost their neuter gender.

At any rate, the Brythonic languages – Welsh, Cornish, and Breton – have only masculine and feminine nouns. An attempt to see one solitary neuter in the Cornish word *tra* 'thing' (Nance 1938: 95) on the grounds that the word is treated as feminine in some grammatical situations and as masculine in others, as is also true of the Breton equivalent *tra*, seems

more than a little far-fetched. Pedersen (1913: 67) regarded the treatment of Bret. *tra* (feminine after the definite article: *ann dra* 'the thing', but masculine after the numerals *daou* and *tri: daou zra, tri zra* 'two things, three things' with the specifically masculine form of the numerals) as an indubitable trace of the ancient neuter. If, however, neuter is to mean neither masculine nor feminine, *tra* seems rather to be both masculine and feminine.

If the neuter gender was the first to be peculiarly vulnerable in Celtic (there seem to have been neuters in Gaulish, Galatian, and Celtiberian, but their history is not known), its place of maximum vulnerability was soon taken in the Brythonic languages by the feminine. (Goidelic shows a similar development. In Old Irish, for example, the feminines were most numerous and the neuters least. In Modern Irish the masculines are about one and a half times as numerous as the feminines). Throughout the centuries the number of feminines has declined, either through loss or through re-assignment as masculines. This is apparent in lexical counts, which are, admittedly, not so important as contextual occurrence. For the lexical distribution of gender, as seen in the relative percentages of masculines and feminines in the dictionary, is obviously of less significance than the frequency with which nouns of each gender are used in speech or writing. If some are used with decreasing frequency until zero is reached and nobody remembers them, they may be said to have vanished from the lexicon, whether by neglect, abandonment, expulsion, retreat, or expiration – and all such words are no doubt largely figurative. In languages lacking a system of writing, the loss is probably irreversible (although the retention of ancient vocabulary, even of 'obsolete' words, may be part of the secret baggage of medicine men and the like, who transmit them to successors, thus resembling on a small scale the use of extinct languages in the rituals of various organized religions.) In literate communities there is a chance of retrieval and resuscitation. Modern German words like *Abenteuer* 'adventure', *Fehde* 'feud', *Halle* 'hall', *Hort* 'hoard', *Recke* 'heroic warrior', *Wonne* 'delight', etc. had once disappeared from the standard language but were revived and re-introduced into the lexicon by authors of the eighteenth and nineteenth centuries. Sometimes the individual author is identifiable and the date of the word's revival known. Through their reading such people were able to recapture part of the lost vocabulary of older periods. Sometimes the revived form has a different gender from the earlier one (*Abenteuer* is neuter, but Middle High German *âventiure* was feminine, e.g.). At times dialects also help in the process of resuscitating once obsolete words, although the semantic direction taken by a word in the dialects was often somewhat remote from the older meaning.

The lexicon is surely something greater than the dictionary, although dictionary makers have often tried to do justice to the lexicon. In Celtic lexicography they have at times done more than that: they have created their own words and flooded the dictionary with non-existent vocables. Part of the task of modern Welsh lexicography, for instance, has been to weed out spurious forms, ghost words, and other such extraneous matter. A quarter of a century ago, when I attempted a preliminary study of gender redistribution in Celtic (Fowkes 1957: 39–46), the best available Welsh dictionary was the twelfth edition of Spurrell's Welsh-English Dictionary (Carmarthen 1934), edited by J. Bodvan Anwyl with the aid of a dozen foremost Welsh scholars. They succeeded rather well in ridding the dictionary of most of the phantoms that had haunted it in the past. And they made admirable progress in distinguishing between obsolescent and obsolete words, and in marking them. That task, however, was not completed to perfection, and the editor practically

admitted that in his preface. For, although a certain number of words could be designated as indubitably obsolete, there was also a sizeable body of borderline words. Poets have from time to time dipped into that store of words and exploited it; moreover other words of biblical origin were not used in everday conversation but were well-known to hosts of speakers. Some of this vocabulary was, with reluctance on the editor's part, marked as not belonging to twentieth-century Welsh or, to use a somewhat eccentric criterion, as being words which the non-Welsh student of the language should not attempt to use in ordinary prose. Those words were marked with a dagger: †. The absence of that sign should have implied that a word was accepted as current Welsh of half a century ago. But many words lacking that mark were not really current after all. Despite such difficulties I compiled statistics on gender distribution based on Anwyl's revision of Spurrell and arrived at the following:

Masculines	7,892.	Percentage of total vocabulary	67.41
Feminines	3,815.	" " " "	32.59

Since the publication of that article a newer Welsh dictionary, has appeared (Evans-Thomas 1978), consulting editor of which was Professor Stephen J. Williams, an outstanding Welsh scholar. First printed in 1958, the work has been through three revisions. The discrimination between obsolete and current words has been exercised with special care, and the work is a more reliable one than all predecessors. (A monumental dictionary – a sort of small-scale 'Welsh Old English Dictionary' has been appearing in fascicles since 1950 (Bevan). By the time this article appears it will presumably have reached letter *M*).

Using Evans-Thomas (1978) and disregarding all obsolete words, I find the following statistics:

Masculines	7339.	Percentage of total vocabulary	71.32
Feminines	2999.	" " " "	28.68.

This contrasts notably with the earlier figures of 67.4% for masculines and 32.59% for feminines.

Exact computations are perhaps not as important as the direction of change. An increase in the percentage of masculines over feminines is clear.

In Breton (Hemon 1973) the lexical figures are:

Masculines	12,711.	Percentage of Vocabulary	56.6
Feminines	9,761.	" " "	43.4

Breton thus exhibits less of a gap between masculines and feminines than does Welsh. But the dominance of masculines is obvious. (Contextually, it is far higher).

Cornish shows closer agreement with Welsh than with Breton, at least lexically. A smaller body of words is available in the dictionaries published (Nance 1938: 95, s.v. *tra*). Figures obtained are:

Masculines	2,205.	Percentage of Vocabulary	74
Feminines	794.	" " "	26.

The picture alters considerably, however, when we count the Cornish nouns in their contextual occurrence in works of literature. I have done this for the four chief works preserved in Middle Cornish. They are: *Ordinale de Origine Mundi* (The Beginning of the World),

Passio Domini Nostri Jhesu Christi (The Passion of our Lord Jesus Christ), *Ordinale de Resurrectione Domini Nostri Jhesu* (The Resurrection of our Lord Jesus Christ), and *Beunans Meriasek* (The Life of Saint Meriasek). The first three were edited and translated by Edwin Norris (1869). The *Beunans Meriasek* was edited with a translation by Whitley Stokes (1872).

These works are all plays in verse. There has been considerable debate regarding their date of composition. The fourteenth century is accepted as the likeliest period by many, while the *Beunans Meriasek* is regarded as somewhat later than the other three, while still in the same century (Longsworth 1967: 7–8; Ellis 1974: 38). The gender statistics for these works follow:

De origine mundi:	masculine	1849 = 83.6%
	feminine	363 = 16.4%
Passio	masculine	1794 = 84.9%
	feminine	319 = 15.1%
Resurrectio	masculine	1517 = 84%
	feminine	287 = 16%
Beunans	masculine	1936 = 85%
	feminine	349 = 15%

The Cornish lexical count, which yielded 70.12% masculines and 29.88% feminines, thus gives an exaggerated impression of the percentage of feminines, which in actual literary occurrence have at least 13% lower frequency than the dictionary might lead us to believe.

Three centuries later than the Middle Cornish works referred to above is *Gwreans an Bys* (The Creation of the World), a miracle play written down by William Jordan of Helston in 1611, but apparently not composed by him (Stokes 1864). Whitley Stokes provided an edition of this play, plus translation, in 1864. The statistics follow:

Masculines	1411 = 86%
Feminines	236 = 14%.

That is not an astounding change three centuries after the other works inspected, although it does show a slight decrease in the percentage of feminine nouns. (One should suspect that it is older than hitherto believed).

The next work used to derive similar statistics dates from the decade 1660–1670. It is a story, "Jowan Chy-an-Horth, py an Try Foynt a Skȳans" ('John of Chyannor [Ram's House] or the Three Points of Wisdom'), written down, along with several other tales (that have not survived), by Nicholas Boson on Newlyn, near Penzance, allegedly in order to provide his children with a knowledge of Cornish (Nance 1949: 37–47). It is, unfortunately, of insufficient length to have much statistical validity, containing only 222 nouns, merely a fraction of those in the Middle and Late Cornish works studied. Of those nouns, 159 are masculine and 63 feminine, i.e. masculines amount to about 72% of the total, feminines to 28%. Hence, approximately four centuries after the Middle Cornish works, but only half a century after the *Gwreans*, if the date is right, there is an actual increase in the percentage of feminines, although masculines still dominate conspicuously. But the extent of this limited evidence dwindles even more when we note that those 159 masculine attestations include only 50 different nouns, the most frequent, *chy* 'house', occurring 20 times; and the 63 feminines include just 21 different nouns, the most frequent, *gwrēk* 'wife', occurring eleven times.

If there is dubious validity of such limited evidence, there may be still less justification for considering writings in 'Recent Cornish', i.e., works composed after the language ceased to be spoken. It might, then, not be legitimate to compare these relatively artificial products with those written in contemporary Welsh or Breton. Since, however, there is a modern Cornish version of the gospel of St. Mark (Smith 1936), I have, possible objections notwithstanding, compared the gender distribution in that book in the three Brythonic languages. What emerges is the following:

Welsh:	masculine 871 (67.5%)	feminine 420 (32.5%)
Cornish:	masculine 1047 (78.4%)	feminine 289 (21.63%)
Breton:	masculine 946 (70.44%)	feminine 397 (29.56%).

Here the Cornish figures, for whatever they are worth, do not show the extremes seen in Middle Cornish, but the masculines continue to dominate, and the feminines are only 36% of the number of the masculines. Furthermore, Cornish surpasses both Welsh and Breton in the number of masculine nouns occurring, as well as in the percentage of masculines over feminines. Now, although the translators of the gospel into Cornish were not using their native language – nor, indeed, a living language – they cannot be suspected of manipulating the vocabulary in order to keep the number of masculines high and the feminines low. In fact, the one possibly conscious act on their part might be the avoidance of excessive English loanwords, and that would keep the masculine count down. At any rate, by the time of the demise, the Cornish vocabulary had reached a distribution of masculine and feminine nouns such that the same passage, translated into all three Brythonic languages, would require far few feminine nouns in Cornish than in the other two.

Why would the Cornish language, in all periods in which it is known, seem to shun the feminine gender more than its cognate languages did (even though the latter also show decline in the frequency of feminines, especially contextually)? Without falling into the trap of teleological interpretation or of attributing to a language the ability to make a choice, we can state that one of the embarrassing complexities of all Celtic languages is the phenomenon of 'initial mutation'. Even native speakers often find that phenomenon a stumbling block. This sandhi-like process (but differing from sandhi in that the precipitating causes are not usually extant, being prehistorical) is, for the most part, more of a burden than a blessing. Feminine nouns induce far more initial mutation than do masculines. (In Cornish there are one or two instances where masculines take a mutation where feminines would not, but these are not of the same order of frequency by any means).

When words are borrowed from other languages, any Celtic language seems to assign nouns of feminine gender in the source language to masculine gender in the recipient language. And when the contributing language (especially English) has virtually no grammatical gender, the chances are far greater for the word to become a masculine than a feminine in the borrowing Celtic tongue. But all of these factors seem to apply to Celtic languages as a whole. Why should Cornish reduce its feminine nouns at a greater rate than the others? The process may be connected with the fact that Cornish is the one Brythonic language that has become extinct. Was this excessive rejection of feminine nouns and the concomitant mutational difficulties part of the process of rushing towards death?

There is considerable doubt as to when Cornish actually died. The story, long current, that Dolly Pentreath of Mousehole, who died in 1777 at the age of 102 – allegedly – was the last native speaker of the language is now treated with deserved skepticism. For there are

reports of native Cornish speakers long after her death (Norris 1869: 466; Jenner 1904: 21–23). Be that as it may, Cornish apparently died some time early in the nineteenth century. As in the case of Manx (which, however, lasted until the 1970's), Cornish has its presentday enthusiasts who read and write the language, and even speak it, to some extent. I have heard a speech given in the Cornish language, and the sentence melody as well as some of the sounds were very 'English', impressionistically. Talk of a Cornish revival can only be called highly optimistic. But some people seem to write Cornish quite capably. A mimeographed quarterly, *An Lef Kernewek* (The Cornish Voice), edited and largely written by E.G.R. Hooper since 1951, has provided excellent Cornish prose and poetry. (A sampling of its issues over the period 1960–1969 shows, incidentally, 76% masculine nouns and 24% feminine). Despite such accomplishments of individual Cornish enthusiasts (including some Englishmen and Americans as well as an occasional German), it has to be admitted that the answer to 'Is the Cornish language dead?' must be in the affirmative.

In a number of admirable studies on the 'dying' Gaelic dialects in East Sutherland, especially that of the village of Embo, Nancy C. Dorian (1973: 413–438; 1976: 96–109) has discussed the role played by initial mutations, chiefly lenition, not in the language death itself but as symptomatic of the approaching end, perhaps. She analyzes the observance or neglect by individual speakers in their treatment of lenition. There is great divergence between the speech of the oldest and the youngest generation. And, although there is a certain determined retention of the initial mutations by all speakers, the younger speakers may use a different ('wrong') mutation, or may use one where their elders have none, or may use mutations interchangeably where the speech of the older folk allow only one, or they may omit a theoretically mandatory mutation.

When failure is perceived in the operation of the system of initial mutations, such failure is on the part of individuals or identifiable groups of individuals. Some short-comings are regarded by other speakers (not committing the same offenses) as hilarious (Dorian 1976: 102); others may pass unnoticed by all but the most competent of fluent speakers. Sometimes fluent speakers themselves are 'guilty' of lapses. I have observed this in Wales. A usually very capable speaker was heard to say several times, of children playing on the beach at Aberystwyth, *Maen nhw wrth ei fodd*, which was intended to mean 'They're having the time of their lives'. But faulty lenition (in this instance using it where none was required) caused the utterance to mean 'They're having the time of his life', and general merriment ensued on the part of young and old alike that so prominent a personage and so fluent a speaker could commit such a 'blunder'.

This article points to a different aspect of the same process: the avoidance of situations in which mutation (especially lenition) would be required, hence the predilection for masculines over feminines, since, as has been said, the latter call for more instances of lenition.[1] That avoidance does not exactly constitute error, because the 'rules' of lenition are observed in those cases requiring them. And the rejection of feminine nouns seems not necessarily to be a conscious act, although it could be in a specific case. But we do not have those cases, since nobody observed Cornish in the process of decay and death; or, if somebody did, no reports were handed down to us. Both at the lexical and the contextual level, however, the process seems to be an act of langue, rather than parole, if one may use those terms in these times. It is possible, of course, that in those instances in which a word formerly feminine became masculine in Cornish, this occurred as a result of errors by individuals to start with, who used the words as if they were masculines. Then the error could

have been repeated so often that a generation arrived on the scene for whom they actually were masculine nouns. In a way, the literary works from which the figures in the present article are derived are the products of individual performance, yet the choice of words must have been motivated by other considerations than the gender of nouns.

In shying away from the feminine gender, Cornish managed to transfer to the masculine a number of nouns with suffixes that once marked feminine gender exclusively, or almost so. For example, the Celtic suffix **-aktā*, which has been exceedingly productive in Celtic languages, yielding *-acht/-echt* in Irish, *-aeth* in Welsh, *-ezh* in Breton and *-eth* in Cornish, was once almost a guarantee of feminine gender. (There is a chance that a small number of such nouns were masculine in Celtic, possibly from **-aktu-*) (Pedersen 1913: 32–33). In Breton such nouns are still almost always feminine, and in Welsh about 92% of the nouns in *-aeth* are feminine (433 out of 473). Cornish sometimes has the old feminine gender, sometimes vacillates between feminine and masculine, but often goes over to masculine gender entirely. Welsh *esgobaeth* feminine 'bishopric' (that *esgob* is a borrowing is not relevant here), corresponding to Breton *eskobiezh* feminine (with a competing form *eskobelezh*, also feminine), whereas Cornish *epscobeth* is masculine (although the first part of the word is obviously more conservative than the corresponding Welsh and Breton forms). Similarly, Corn. *cudhyjygeth* 'contrition' is masculine, while Breton *keuzidigezh* (from which the Cornish word is possibly taken) is feminine.

Cornish has been aided and abetted in this shift away from the feminine gender of nouns in *-eth* by the two-fold origin of *-eth*. It can be, like Welsh *-aeth*, from Celtic **-aktā*, (cf. Dorian 1973: 413–438; 1976: 96–109), or it can be the equivalent of Welsh *-edd* (from Celt. **-i̯ātā*, which may have been feminine in origin but came to mark masculine nouns too.) Eventually the masculines in *-edd* outnumbered the feminines in Welsh. Cornish borrowed some of these, and additional support for *-eth* was provided as a masculine termination in Cornish. Thus, Corn. *gwyryoneth* 'truth' is masculine, like Welsh *gwirionedd*, and the feminine gender of Breton *gwyrionezh* is no doubt a retention of the early gender (reinforced by *-ezh* from **-aktā*).

A number of loans of Norman origin, all apparently feminine to begin with, have entered Cornish, perhaps at more than one period, and seem to be entirely masculine in their Cornish guises, for example: *benedyksyon, condysyon, conversasyon* ('manner of life'), *crēasyon, dampnasyon, devosyon, examnasyon, nasyon, oblasyon, passyon, pensyon, redempsyon, remosyon, salvasyon*, etc. When there are Breton equivalents they are feminine; Welsh ones vary, but feminines predominate.

A large number of other nouns which seem to be ultimately of French origin have masculine gender where French has feminine. Some examples are: *antyquyta, auctoryta, marchondȳs, lēouta* ('loyalty'), *rēouta* ('royalty'), *cryjyans* ('credence'), *aquytans, venjyans, mūsyk, arsmetryk* ('arithmetic'), company, etc., all of which are masculine in Cornish. The nouns in *-(y)ans* show a suffix which accidentally coincides with a native one, *-(y)ans* of Celtic and even pre-Celtic origin. It is more productive in Welsh, apparently, than in Cornish, cf. Welsh *colliant* 'loss', *gogoniant* 'glory', *gwelliant* 'amendment, betterment', *llwyddiant* 'success', *peiriant* 'engine', etc., all of which are masculine. Cornish *gordhyans* masculine 'worship, honor' is an instance of a native word with the same suffix, as is, most likely, *arghans* masculine 'money, silver', Welsh *arian*, Middle Welsh *ariant*, Breton *arc'hant* masculine. It is conceivable that the native ending, associated as it was with the masculine

gender in Brythonic, provided the analogy for assigning that gender to the French loanwords. It is difficult to decide whether these words came into Cornish directly from Norman French or were transmitted via Middle English. If they were received from English at a stage that knew no grammatical gender, it is not surprising that Cornish made them masculine. Many of them seem to have been learned borrowings (ecclesiastical, especially) and in such cases there could conceivably be restoration of the original French gender, but that seems not to have occurred. (That the Breton gender matches the French is not surprising).

At times gender distinctions are useful in distinguishing homophones, cf., for example, German *das Tor* 'the gate' vs. *der Tor* 'the fool'; or *der Leiter* 'leader' vs. *die Leiter* 'ladder'. Many pairs of such homonyms could have been similarly differentiated in Cornish, yet that advantage was often relinquished in favor of reducing the number of feminines. Corn. *owr* 'gold' and *owr* 'hour' are both masculine, although the second noun is from an earlier feminine. There are many such pairs: *ancar* masculine 'anchorite, anchor'; *ēr* masculine 'defiance, eagle'; *cost* masculine 'cost, coast'; *porth* masculine 'doorway, port'; etc., although sometimes an old distinction is retained: *toll* masculine 'hole' vs. *toll* feminine 'toll'.

If studies by Dorian (1973: 413–438; 1976: 96–109), Dressler (1977: 33–34), Szemerényi (1981: 281–310), Swadesh (1948: 226–235), and others have shown that certain losses, confusions, and simplifications are symptomatic of imminent language death, it must not be assumed that the symptoms cause that death. Nancy Dorian cogently presents her arguments that the failure of differential lenition in a dialect of East Sutherland Gaelic is one type of change reasonably to be expected in 'decaying' and 'dying' Celtic dialects. Other significant changes occur in morphology, too, for example the loss of the genitive. The excessive favoring of masculine over feminine gender in Cornish is part of a 'failure' to observe many occurrences of lenition that would once have been mandatory. It is pointless to speculate whether Cornish might have achieved a state of no grammatical gender (like English, the overpowering neighbor, and possibly somehow because of that neighbor?) had it survived. It is also mere speculation to guess that Welsh may be the next Brythonic language to become extinct (since Breton has not ravaged its feminine nouns to the same extent). It is also not to be taken as a sign of the approaching death of any language when loss of grammatical gender occurs (English and Armenian refute that). Nor would wholesale elimination of mutations in a Celtic language necessarily herald the death of that language (although something similar to it is seen in the symptoms associated with apparently imminent death in the Embo dialect studied by Nancy Dorian). In recent decades a type of Welsh has come to be favored as something like a colloquial standard. Called *Cymraeg Byw* ('Living Welsh') (Fowkes 1981: 107–118), it has simplified complexities, eliminated some superfluous differentiations, and disentangled many intricacies. It has not abolished the feminine gender, and it has not ruthlessly assailed the mutations. Its proponents hope that *Cymraeg Byw* will help preserve the language. Time will tell.

Oswald Szemerényi (1981: 285–286), in discussing Wolfgang Dressler's studies of certain Breton dialects, many youthful speakers of which seem to disregard to a great extent the mutations (a situation that might point to a rapid decline), makes the perspicacious observation that the prediction could easily prove false if the revived nationalistic enthusiasm evinced by younger Bretons at the present time turns out to have a positive effect upon the question of the language and to reverse the trend seeming to threaten the language with extinction. Such a reversal obviously did not occur in Cornish. It may very well do so in Welsh.

Dynargh, Henry Melgos! Mester yethow hag yedhyoneth, dysgador, scoler, densa, car. Dew re-sowenno dheugh-why hag bedheugh-why lowenek y'n bledhynnow a dhe.[2]

Notes

[1] Lenition in Cornish: replacement of a voiceless stop by the homorganic voiced correspondent: *p t k* become replaced by *b d g*; the voiced stops become corresponding voiced spirants *v* ð *γ* (the latter realized as zero), etc.

[2] Reluctantly rendered into *Sawsnek* thus: Greetings, Henry Hoenigswald! Master of languages and linguistics, teacher, scholar, human being, friend. Godspeed and every happiness in the years to come!

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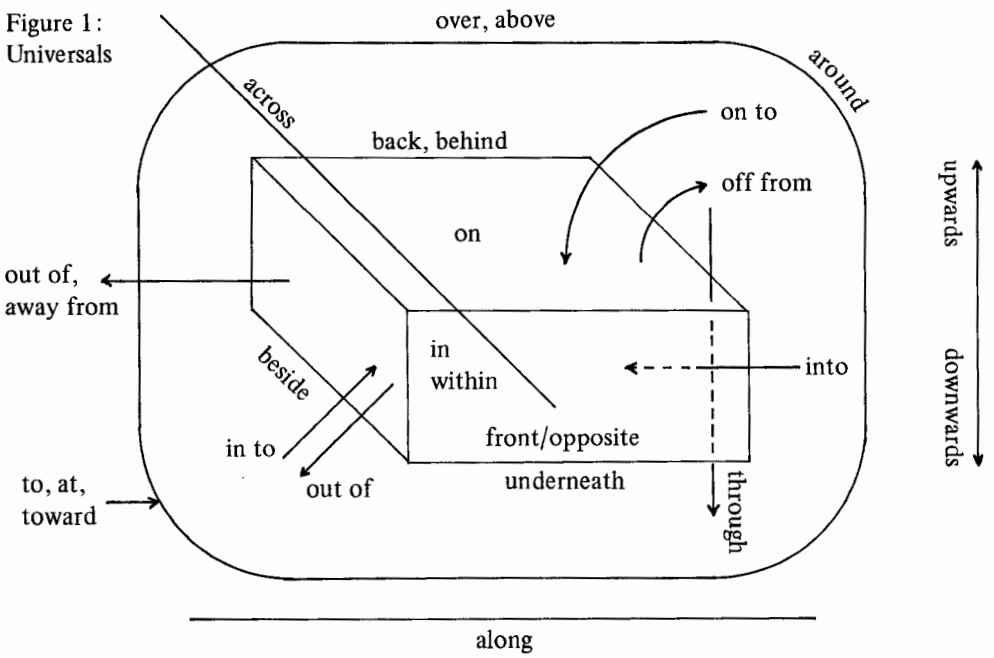
The Proto-Indo-European adpreps*
(Spatio-temporal auxiliaries)

Paul Friedrich (University of Chicago)

Universals

All ideas about relations are implicitly spatial, but some relational ideas are primarily so – and of a concrete space at that. An irreducible minimum of such spatial categories is integral to every morphology and syntax; it helps organize and constitute many fields of meaning (of which it is a part); it is indispensable for interrelating the ideas in a text, and for getting done the practical jobs of language: 'Stand up!' Like many if not most radical or basic ideas, these spatial ones can be pointed at, gestured, or similarly represented (Friedrich 1979: 452). The set is mapped below in Figure 1, as is consonant with its universality.

Figure 1:
Universals



Also: between, among, with

* For their excellent comments on this paper I stand indebted to Phil Baldi, Rani Fedsin, Deborah Friedrich, Paul Hopper, Bruce Mannheim, and James Redfield (the latter also tabulated the Homeric Greek occurrences on the figure – about 1975).

Particulars

Universal spatial ideas are realized in the most diverse wavs. In the Tarascan language of southwestern Mexico, for example, most such spatial relations are expressed by suffixes, most of which have body parts as their primary meaning, be this as suffixes in long verbal words, or as constituents of adverbial units: *ču* 'bottom, buttocks, underneath' (Friedrich 1971: 21 et passim); analogously, 'inside' and 'chest, thorax' are symbolized by another suffix. Some Hittite and even Proto-Indo-European adverbs and prepositions decidedly originated from words for body parts – for example, 'heart' and 'inside, among' (Ivanov 1973: 56–57); Wackernagel (1916: 182) thought that nominal bases could be assumed for all such units, although some were probably deverbal (e.g., *tr* 'to cross' yielded *trans* 'across').

Depending on the language, spatial-directional relations may be expressed through case endings, enclitics, particles, idioms, phrases, phonaesthesia, or the implications of syntactic constructions (e.g., when they involve analogues between temporal, spatial, and transitivity values). A most common device are the units known as adverbs or prepositions or others that to a significant degree play the role of both these parts of speech – in short, the "adpreps" (Bolinger 1971); the partly overlapping term, "adposition," alludes only to morphological slot.

Greek In Homeric Greek these adpreps number eighteen (the postpositions -δε, -θεν, and -φι are not included here). They are distributed throughout the texts and are frequent, although some are more common than others; the first 250 lines of the *Iliad* contain all eighteen, but the rate, or range in frequency,.is from two that only occur once (ὑπέρ, περί), to ἐπί, which occurs 22 times (as is consistent with its least-marked status). A full list of the Homeric Greek adpreps is given in Table 1 in the order of their appearance in *Iliad* I; the first number after each adprep refers to the line in which it first occurs; the second number refers to the number of times it appears in the first 250 lines of the *Iliad*; these are the usual 'proper' prepositions.

Table 1

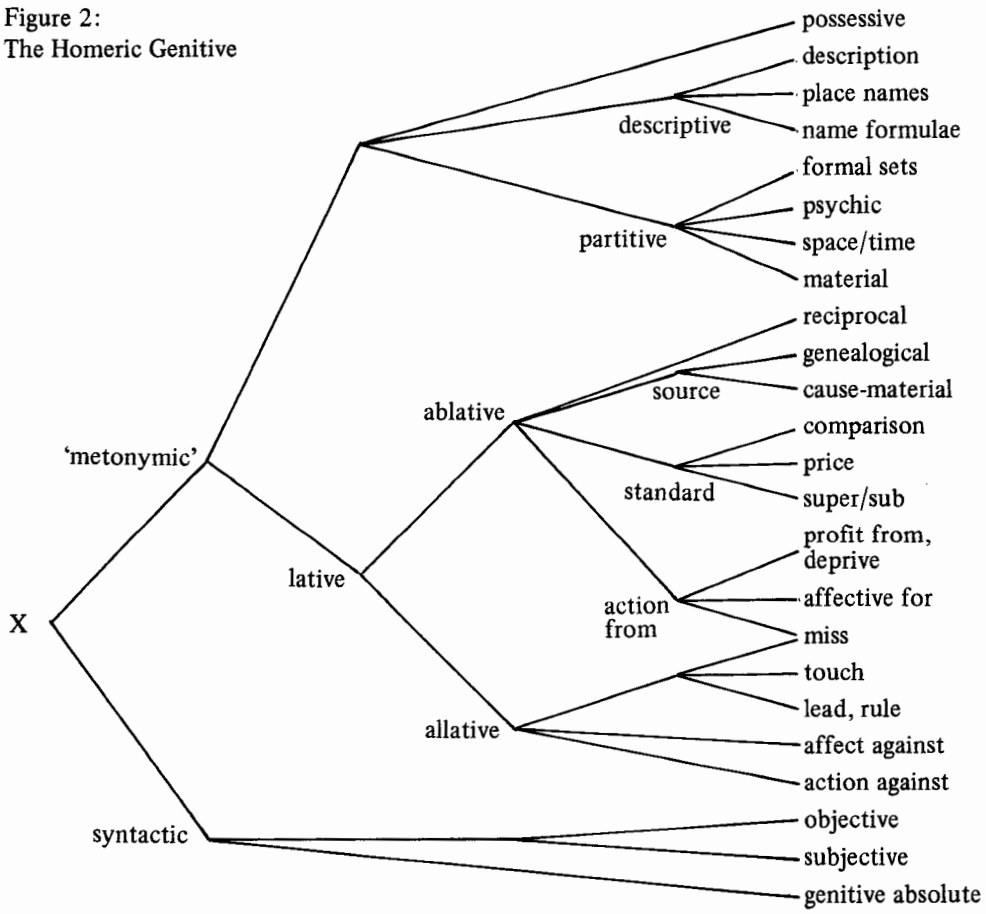
πρό	3	8	παρά	34	6
ἐπ/ἐξ	6	12	ἀμφί	37	5
διά	7	4	κατά	40	7
σύν/ξύν	8	5	μετά	58	8
ἀνά	10	9	εἰς/ἐς	71	5
ἐπί	12	22	πρός/ποτί/προτί	84	9
ἐν/ἐνί/εἰνί/εἰν	14	9	ὑπό	147	2
ἀπό	25	20	ὑπέρ	205	1
ἀντί	31	3	περί	230	1

What is the meaning, syntactic behavior, and status of these adpreps in Homeric Greek and, to some extent, in Proto-Indo-European?

In Homeric Greek and other early Indo-European languages the adpreps may occur in phrases that range from hapax legomena to frequent formulae. The adnominal adpreps, to begin with, stand in two functional relations. First, they may disambiguate case categories: for example, the genetive of source versus the ablatival genetive of motion away from. Second, their own spatial meanings may be disambiguated by case – if an adprep can occur with more than one case (Kuryłowicz 1964: 176).

In fact, only εἰς goes only with one case, the accusative; otherwise, two adpreps go with the genitive only (e.g., ἐκ), two go with the dative only (e.g., ἐνί), four go with the genitive or the accusative (e.g. διά), and no less than eight go with any of the three oblique cases – with differing semantic consequences, of course. In such multiple options, one of the co-occurring cases reinforces the primary meaning of the adprep, whereas the others are 'exponents of a secondary function,' or 'specify the spatial relations expressed by the case form' (Kuryłowicz 1964: 178). Some intimations of the overall semantic-syntactic complexities are conveyed by Figure 2 of the subcategories of the Homeric Greek genetive, which – in interaction with context – is often disambiguated by an adprep. (The so-called 'improper prepositions' such as ἄγχι all take the genitive, but, on the other hand, are not preverbal).

Figure 2:
The Homeric Genitive



We find considerable freedom in order. The adnominal adpreps may be prefixed to substantives, either as mobile, detachable units, or as part of frozen compounds. They usually precede the noun, but may follow (the reverse of the Sanskrit rule). They often occur between adjective and noun, as in θοὰς ἐπὶ νῆας, and this is a favored formulaic construction. They are never formally necessary, as in τόξ' ὤμοισιν ἔχων, "bow [on his] shoulders having (Wackernagel 1916: 197). But the rules and patterns for all these adprep-noun co-

occurrences, which are largely stylistic, have never been stated succinctly, and it would be extremely difficult to do so.

As is clear already, cases also vary greatly in the degree to which they need an adprep. The more sensitive the case is to context, the more rigid its binding to an adprep. Other early dialects have stricter rules. In the Rigveda the fourteen 'genuine prepositions' are 'almost entirely restricted to employment with the accusative, locative, and ablative. . . and were originally connected with the accusative, locative only' (Macdonnell 1971: 210). Latin is relatively rigid when it comes to these and other case relations (Baldi 1977).

The degree of overt, linear attachment to the verb runs a wide gamut from (1) direct prefixation to a bound verbal form, to (2) direct prefixation to a free verbal form, to (3) detached from the form but directly preceding it, to (4) separated by one word only, to (5) separated by two or more words, to (6) postposed directly after the verb, to (7) separated by two or more words from the preceding verb (in both (6) and (7) the adprep gets the stress), to (8), where the adprep and the verbal form are in adjacent lines. When not directly prefixed, the meaning of the adprep and, hence, of the expression in which it occurs (or simply of the rest of the expression), may remain unaffected; that is, AdprepVerb=Adprep . . . Verb, as in *οἱ κατὰ βοῦς ἠσθίον*. In other cases, the discontinuity may decrease the bonding of the adprep with its verb and increase its bonding with a noun. Often a new ambiguity is created; thus, *περιαιρέω* means 'to strip off,' but *περὶ φρένας ἵμερος αἵρει* seems to include this meaning and also something like 'seized all around,' that is, 'desire seized his heart all around.' In other cases there are subtle semantic differences reminiscent of English 'to cut the bread up, to cut up the bread.' Other things being equal, the farther the adprep is from its verb the greater is the likelihood of changes in meaning of the sort just adumbrated.

When primarily bound to a verb, the adprep, in what is its second main function, disambiguates the aspectual subcategories of the verbal expression. By 'aspect' is meant the temporal values inherent in the activity, event, or state itself (Jakobson 1957: 493; Holt 1943), particularly as regards the features of duration, instantaneity, inception, completion, and boundedness. In Homeric Greek this aspect was of at least four main kinds, some more powerful than others but never enough so that, logically, one can include the other. The subcategories are given here in their order of power and of textual frequency.

First, an adprep can denote or at least adumbrate the relative instantaneity or punctuality, as against the durativity, of an event or act 'to sail along (*παρά*) the shore.' Second, an adprep can denote or imply completion (or lack of totality): 'to cut off' (*ἀπο-τάμνω*), 'to cut' (*τάμνω*). Third, several adpreps denote direction toward a goal or simply direction (telic or determinative aspect): *πρός*, for example. Finally, several adpreps denote realization or the full result of an action. In all four of the meanings just given the adpreps are in some sense bounding the action, as opposed to not bounding it (this category of bounding in aspect theory is best argued by Jakobson). The strong case for boundedness as the deepest semantic feature is, of course, more or less congruous with the pragmatic or discourse-oriented claim that these are perfectivizing mechanisms to provide for sequencing, for closing off one event before another can begin to be narrated.

The aspect value of adpreps is often, perhaps most typically, actuated with verbs of motion. Moreover, the subcategories interact with each other and two of them are often realized simultaneously in one form; thus, the telic often interacts with the durative/

punctual or the completive/noncompletive. The adprep *σύν* is particularly interesting because it usually has the telic meaning of 'the reunion of objects' (Brunel 1939: 107), as in 'to tie together, to come together.' But this may combine with the aspect of the verbal root (*Aktionsart*) to denote completion, as in 'I shatter completely' (*συνάγνυμι*). Latin *per* is also used this way. Here are the first six adpreps with examples of some of their aspectual functions (Table 2).

Table 2

ἀμφί	intensifies durative value of *ἀμφιπένομαι* 'to be busy at'	durative
ἀνά	*ἀναρπάζω* 'I snatch up'	punctual
ἀπό	*ἀποτάμνω* 'I cut off'	completion
διά	*διαπέρτω* 'I destroy utterly, sack'	realization
εἰς	*εἰσαφικάνω* 'I arrive at a destination'	telic
ἐκ	*ἐκγελάω* 'I laugh out'	inceptive

Among the variables governing the relations between adprep and verb one must include the following: (a) the (degree of) boundedness of the verb; (b) the degree of attachment between adprep and verb; (c) the degree of (allowable) distance between adprep and verb; (d) the freedom of the adprep to occur after the verb; (e) the number of adpreps that have the freedom of distribution to occur with a given verb; (f) the number of verbs that are free to occur with a given adprep; (g) the changes in meaning – including subtle nuance – connected with adprep association; (h) the number and identity of adpreps that can occur with a given verb at the same time (which runs up to three); (i) the bondedness of the adprep with the nouns and their case meanings in a phrase or other construction; (j) the interaction between the adprep and its noun, the adprep and its verb, and so forth. Actually, an adequate understanding of how adpreps work would call for delicate measures and exhaustive lexicography of the relative affinity between specific adpreps and whatever they were in construction with, both in general and in the sense of specific verbs – granted the high degree of idiomaticity and unpredictability and, indeed, the low feasibility of the entire project. (Cunliffe and similar lexicographers are excellent on the scope and, to some extent, the frequency of these co-occurrences, but there remains the problem of combinatory potential; compare Friedrich (1971), where an attempt is made to give the combinatory potential of all Tarascan verbal roots with all suffixes.)

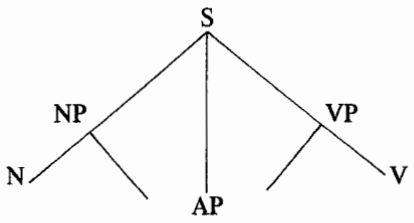
The complexities and realities of the adprep systems are obscured in many ways by the conventional conceptualizations and nomenclatures. Adpreps, for example, actually occur more often unattached, and this is the unmarked order. Rather than saying that the unattached adpreps are in tmesis, which would be correct for Attic Greek or Old Norse, we should use a special term for the marked, attached adpreps – perhaps 'in synthesis.' Similarly, the traditional nomenclature of adverb, preverb, preposition, and postposition has – except where these are used for special situations – obscured the more basic reality that the adprep is always typically in construction with both the verb and the nominal object, as most or at least many Homerologists and Indo-Europeanists have recognized (Meillet, Benveniste, Ivanov, Chantraine). The adpreps are underlying verbs – specifically auxiliaries – and can, with the verb in ellipsis, function as surface predicates, as in *ἄν* *Ὀδυσσεὺς* *πολύμητις*.

Syntactic Trees Here are two typical examples of the sort of construction in question:

ἀνὰ στρατὸν ὤχετο κῆλα θείοιο
'the arrows [*κῆλα*] of the god [*θείοιο*] went [*ὤχετο*] against [*ἀνὰ*] the army [*στρατὸν*]'
ἀμφὶ γὰρ ὤμοισιν βάλετο ξίφον
'about [*ἀμφὶ*] his shoulders [*ὤμοισιν*] he threw [*βάλετο*] his sword [*ξίφον*]

I assume that the meaning of the adpreps is to be found, not only in the forms, but in the way they combine with other units in phrases and clauses in stanzas, episodes, similes and yet other discourse-length spans. How are the sliding scales of governance to be represented syntactically in terms of trees, bracketings or some other explicit, if necessarily simplified form?

There are four main kinds of construction to be represented. In the first the adprep is primarily in construction with the noun and both are subordinate to a verb (Kuryłowicz's "first type"). In the second, equally important type the adprep is bound mainly to the verb, on which it is dependent; for example, [NP [Adp V]] (assuming a NV order). Third, the adprep is in construction with the verb in the verb phrase, and, indirectly, with a noun phrase subordinate to that verb phrase. Last, the adprep is simultaneously in construction with both the verb (phrase) and the noun (phrase). This is the least-marked proto-syntagm, the syntactic archetype. The problem of its looseness will be taken up below.



Meaning

The primary meaning of the Homeric Greek adpreps was spatial: *σύν* 'with' (that is, side by side), *ἀπό* 'off, away,' and so forth – spatial meanings such as are laid out on Figure 1 above. But the meanings, as shown above, are often temporal as well; as Chantraine says (1963: 82), they are 'little invariable words, originally autonomous, which make space-time relations more precise.' These space-time markers are qualitative, never quantitative (whereas the noun (phrases) in an aspectual unit often are quantitative); but the way the adpreps work to express at least a dozen subcategories of aspect is congruous with the multifarious way aspect is quantified through adverbs or nominal adjuncts. Incidentally, the same technical term seems to have been coined independently about the same time by Ivanov (1973: 60: *sluzhébnoje postránstvennoje ímja*) and Friedrich and Redfield (1973–74: locative auxiliary); however, 'spatio-aspectual' now appears more precise than 'locative,' and 'adprep' is not only conveniently short but conveys more information than 'auxiliary'.

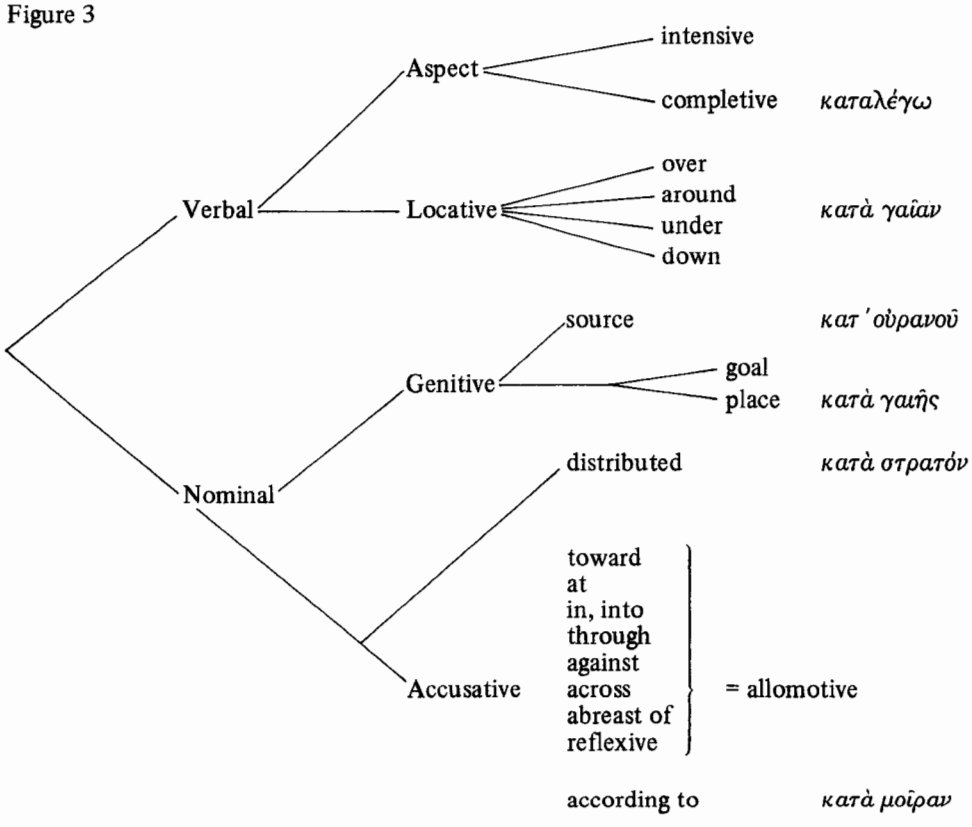
The semantics of the adpreps entails other theoretical dimensions. For one thing, there are important semantic relations between aspect and case. The latter denotes what are, at least implicitly, spatio-temporal ideas; for example, away from a space (ablative), into a space

(accusative). Such meanings are variously congruous with aspectual ones: the accusative case with the telic aspect, the nominative with the durative, and so forth. The Homeric adpreps, finally, are interstitial between grammatical ('pure relational') elements, such as affixes – since they are always potential prefixes – and, on the other hand, the more concretely referential words in the language (Sapir 1921: 107). This structural interstitiality or hybrid status may be one reason they have been neglected or misclassified as a set in many general books on Proto-Indo-European.

Adpreps in Contexts

The aspectual value of the adpreps should not be neglected, but it also should not be exaggerated: these meanings are far less powerful than the spatial ones. Let us take a fairly typical Homeric Greek adprep, *κατά*, which has a strong cognate in Hittite *kata*, and a Proto-Indo-European etymon. Meanings that are primarily or exclusively aspectual define only two out of eleven or, by another count, 2/18 of the categorial total, and its total aspectual role in texts would also probably come to about one fifth of the total; the following Figure 3 summarizes some of these patterns. Readers desiring the full, actual contexts need only look up in Cunliffe each of the forms cited in this figure and go from there to the Homeric Greek loci.

Figure 3



Proto-Indo-European

Homeric Greek is a well-marked stage in the transition from Proto-Indo-European and Pre-Greek to the typologically much changed structures of Attic and Hellenistic Greek (Meillet 1934). The complex drift had several interconnected components that are integral to the adprep question. First, there was case syncretism, the ablative fusing with the genetive, and so forth. With the loss of the ablative, locative, and instrumental cases, the functions and frequencies of the adpreps increased, and the adpreps 'proper' tended to be replaced by 'improper prepositions,' and by new adverbial forms derived from nouns. Second, the adpreps fused with specific verbs in the process of 'univerbation,' so that eventually an adprep was never free of its verb. Third, word order became much more fixed; adpreps, for example, came regularly to precede their nouns. Fourth, the adpreps came to agree more regularly with the more limited number of cases, particularly the accusative (just as certain conjunctions became associated more rigidly with certain moods through rules of taxis).

From these well-documented, interdependent, multivariate drifts (Sapir 1921: VIII), we can extrapolate backwards toward Proto-Indo-European, and posit adpreps that were relatively autonomous and unattached and, concretely, about equally in construction with nouns and verbs (Wackernagel 1916: 165, 178 et passim). A full panoply of six of the eight cases were variously and loosely in construction with the adpreps, as were various aspectual forms of the verbs. These hypotheses, which seem sound on the basis of Greek alone, are strongly supported by other early dialects, notably Vedic and Hittite. In Vedic, "the place of the preposition is free . . . in that, in principle, it can appear before or after the noun" (Renou 1968: 169–70). Similarly, "adverbial prepositions follow, but also precede, their case (Macdonnell 1971: 208). Of the cognate situation in Hittite, Pedersen wrote (1938: 151): "Es hat im Hettitischen insofern ein altertümliches Aussehen, als die meisten Proverbia noch dem Verbum gegenüber sehr selbständig sind, also eigentlich Adverbia sind, bei denen es im Einzelfall oft unmöglich zu entscheiden ist, ob sie sich einem vorhergehenden Kasus also Postposition anschließen oder als Praeverbia zum folgenden Verbum gehören." See also the more recent statement by Otten and Soucek (1969: 86–88).

In sum, adprep rules were relatively probabilistic. Adpreps occurred about equally before and after nouns in Proto-Indo-European (with markedness inconclusive). Vis-à-vis verbs the least marked position was probably the preverbal one, although they were free to occur postverbally. The most marked position was probably clause initial. My hypothesis is that most of the adpreps could and usually did stand in construction with both nouns and verbs, but that a few were mainly adnominal while a few others mainly modified verbs; some, at any given point, were in various stages of syntactic change and differentially bonded and with differential freedoms of distribution. I think we have to assume that the meaning of the adpreps would – depending on the type of bonding – fluctuate considerably in degree of abstraction and transparency. Finally, it would in principle be possible to construct a markedness diagram for all 23 Proto-Indo-European adpreps (granted the prodigious problems of coding, multivariate analysis, and lexical detail needed for the exposition). We are left, for now, with the archetypal formula given above as the most general and accurate description.

Indo-Europeanists tend to attribute phonological and morphological contrasts back to the reconstructed parent language (for example, the voiced aspirates, the eight cases). Then we derive the forms of the descendent languages by loss and simplification – or better, what I

would call symmetricization (interacting with complication through analogy, and so forth). It is interesting that – by what looks like the obverse of the same assumptions and procedures – the tendency is to posit a relatively simple, loose, and flexible proto-syntax (e.g., Meillet 1964: VII – for a contrasting argument see Lehmann 1974). But there is a real difference between the phonological and syntactic situations and the attendant problems of reconstruction. A phonology at an earlier stage (e.g., Old English) is *not* generally more complicated than at a later stage (e.g., Modern Texas English), whereas the syntax of many of our texts in early Indo-European *is* demonstrably looser in the sense, for example, that optional rules are more powerful and marked forms more frequent. The syntactic looseness in question might well be expected in the extremely poetic texts of such languages as Homeric Greek and the Rigveda (Wackernagel 1968: 88 et passim thinks such looseness is one diagnostic feature of poetic language). But it also seems likely that the language of ordinary discourse (conversation, narrative, and so forth) was more poetic in Proto-Indo-European times. This point need not be over-played, however: many of our early texts are not particularly poetic (e.g., in Hittite, Old Persian, Old Latin) and we also depend on their relatively prosaic language for inferring the meaning and syntactic function of, inter alia, the adpreps.

As for their meaning, the Proto-Indo-European adpreps were originally primarily spatial (Wackernagel 1916: 210), although there was great dialectal variation (Wackernagel 1916: 154). They were also aspectual, and many of them contributed a perfective sort of meaning (Wackernagel 1916: 181, also Chantraine 1903: 40), whereas others denoted various other categories and subcategories of aspect. These ancillary aspect functions resemble each other in our most critical texts: Homeric Greek, the Rigveda, Hittite, and Old Irish; the texts in Old Russian and other languages are also often priceless.

The actual phonemic form of the adpreps has been or can be reconstructed with as much certainty as can any other part of the early vocabulary or morphology, and is thoroughly documented in the major dictionaries by Frisk, Pokorny, and the like. The forms are set forth on the table below (I don't know that they have ever been brought together before as a set). The layout runs: the proto-form, then the main spatial meanings, then one or two hypothetical aspectual meanings, then examples from three or four stocks, and then the total number of stocks with cognates for the form in question. Incidentally, one reason that the inference of the aspectual value of the Proto-Indo-European adpreps must remain hypothetical is that it is so hard to reconstruct syntagmata with unbound adpreps. The meticulous work by Kuhn, Wackernagel, Schmidt, and the like, has never to my knowledge included a syntagm with an adprep – to say nothing of a free adprep. This reflects one limitation of the comparative method. In many cases there are several reconstructable forms with overlapping or roughly the same meanings (e.g., many of Pokorny's entries), and it is difficult to work out a hierarchy. Lehmann (1974: 232) lists eight such cognate forms for Proto-Indo-European **pro*; in some cases these sets can be ordered in terms of inflection and/or ablaut (e.g., *pro-, pre-, pero-*; (Meillet 1964: 350)).

Table 3

**ad*, 'to at, towards, by' towards, by'	telic	Lt. *ad*, OIr. *ad*, Go. *at*	4 stocks
**ambhi*, 'on both sides, around'	perfective, durative	*ἀμφί*, Osc. *ampt*, Alb. *mbi*, OIr. *imb-*, Ger. *um*; *ἄμφω*, Lith. *abu*, Toch. A/B	7 stocks
**an, anu*, 'up, upwards, along'	ingressive, telic egressive	*ἀνά*, Av. *ana*, Go. *ana*, Arm. *am-, ham-*	5 stocks
**anti*, 'facing, opposite'		*ἀντί*, Lt. *ante*, Hitt. *anda*	4 stocks
**apo*, 'away, off'	inceptive	*ἀπό*, Sk. *ápa*, Go. *uf*, Old Cornish *a*	6 stocks
**dis-a*, 'through'	realized	*διά*, Lt. *dis*, Go. *dis-*, Alb. *tsh-*	4 stocks
**do*, 'to'	telic	*-δε*, OE *tō*, OIr. *do*, Russ. *do*	3 stocks
**eḱs, eǵhs*, 'out of'	inceptive,	*ἐξ*, OIr. *ess-*, OCS *iz*, OPr. *esse*	6 stocks
**en*, 'in, into'	liminal	*ἐν*, Arm. *i*, ON *ī*, Av. *nī-*, Toch. A/B *y-, yn-*	10 stocks
**enter*, 'into, between'	liminal	*ἔντερον*, Arm. *ənder-k'*, ON *idrar*, Lt. *inter*	9 stocks
**epi, opi*, 'near on'	telic, inceptive	*ἐπί*, RV *api*, Hitt. *apa*, Irish *Ériu*, 'Ireland' (*epi-ueriō)	9 stocks
**et*, 'away, beyond'		*ἔτι*, Lt. *et*, Toch. A *atas*, OCS *otŭ*, Sk. *áti*	5 stocks
**knta*, 'down'	completive	*κατά*, Hitt. *kata*, OIr. *cĕt-*	3 stocks
**medhi*, 'middle, among'	telic(?)	*μετά*, Alb. *mjet*, Go. *mip*, OCS *mežda*, Sk. *mádhyu*	6 stocks
**ni*, 'downwards'		Sk. *ní*, Av. *nī*, OHG *nidar*, OCS *nizъ* (maybe Gk. *νειός*)	4(5) stocks
**para*, 'beside, along'	durative	*παρά*, Lt. *prae, por-*, Go. *faúr*, Sk. *purá*	5 stocks
**peri*, 'across, around'	durative(?)	*περί*, Sk. *pári*, Go. *fair*, Lith. *per*, Russ. *pere-*	6 stocks
**pos*, 'behind, right by'		*πός*, OCS *pozъ*, Lith. *pàs*, Alb. *pas*, Lt. *post*	7 stocks
**pro, pero*, 'before, ahead of'		*πρό*, Go. *fra*, Sk. *pra*, OIr. *ro*, Hitt. *paraa*	7 stocks
**proti*, 'to, against'		*πρός*, *προτί*, RV *práti*, Lt. *pretium*, OCS *pretivъ*	4 stocks
**s*, 'with, side by side'	telic	*σύν*, *ξύν*, OCS *cъ*, Lith. *sù*, Myc. Gk. *ku-su*	3 stocks
**tras*, 'across'		RV *tirás*, Av. *tarō*, Lt. *trans*, OIr. *tar*	3 stocks

**uperi*, 'over, out'		*ὑπέρ*, Av. *upairi*, *i ver*, ON *yfir*, OIr. *for*	7 stocks
**upo*, 'underneath, from behind'	telic	*ὑπό*, RV *úpa*, Go. *uf*, Myc. Gk. *upo*, Lt. *sub*	

Naturalness and Nuance

That aspect is a natural ancillary value of the spatial adpreps is supported in other ways. In one part of the Indo-European area, we find that the English prepositions enter into periphrastic constructions to yield a panoply of phrasal compounds with a rich semantic texture that is partly aspectual. In a very different part of the Indo-European area, both geographically and typologically, we find the cognate Russian adpreps functioning adnominally, and centrally, of course, in the aspect morphology. The aspect of the adpreps in Russian and English could be illustrated just as well by Hindi or Welsh or any other Indo-European language – or any language with such components (and I know of none that entirely lacks them). The burden of proof, in short, rests with the proponents of the null hypothesis: that the Proto-Indo-European adpreps did not have aspectual functions.

The aspectual information in a complex verbal form is never exclusively binary, even in Russian, but always involves a plurality of aspect values in a network of aspect values. These values vary enormously from one adprep to another: some are 'exclusively' (i.e., almost exclusively) binarily aspectual in a specific case (e.g., the *s-* of *spet'*, the perfective aspect of 'to sing'). In other cases various degrees, clines, and intersections are involved, or it is a matter of nuance, and no Russian adprep lacks such nuances. In English the adprep compounds similarly involve all manner of nuance, with meanings that range from fairly simple lexical ones to others so subtle that only a native speaker can command them, that is, use them without semantic accent (e.g., 'to rip, to rip up, to rip through, to rip off,' etc.). The same appears to be true of all natural languages and the assumption of such natural nuance seems realistic and fruitful for Proto-Indo-European studies.

Conclusion

Ultimately, the universal scheme mapped at the outset is adequately labelled by the full set of Proto-Indo-European forms on the final table above, forms which were used to communicate about physical space and interpersonal relations, and to handle communicationally indispensable aspect categories in the verbal system. I have assumed that Proto-Indo-European was a natural language, which means at least two things. First, it was like any known language with respect to syntactic form, pragmatic needs, semantic values, and so forth. Second, naturalness also means that the actual form-meaning units, here the adpreps, were adumbrated and complicated by language-specific nuances that are harder, but not totally impossible to recapture; such nuances of aspect might, felicitously, constitute the subject of a sequel analysis in the next festschrift for Henry Hoenigswald.

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Regular contact dissimilation

Hans Henrich Hock (University of Illinois)

1. Ever since Osthoff and Brugmann's (1878) 'neogrammarian manifesto', historical linguists commonly have distinguished between 'regular' sound changes (such as assimilation, weakening, and non-dissimilatory loss) and 'sporadic' ones (such as dissimilation, metathesis, and haplology). Osthoff and Brugmann explained this distinction by considering regular change to be 'mechanical', only phonetically conditioned, but sporadic sound change to be conditioned by 'psychological' factors. These latter were identified by Brugmann (1909) as the "horror aequi(voci)", the avoidance of repetition. As time progressed, also distant assimilation – except for vowel assimilation in neighboring syllables – was found to be sporadic. Apparently for this reason, Paul (1920) invoked a broader concept to distinguish between regular and sporadic change, namely that the latter is conditioned by the same factors which are responsible for speech errors or slips of the tongue: incorrect monitoring of the sequencing of speech events and the difficulties involved with speech production which are exploited in tongue twisters.

2. It is the merit of Henry Hoenigswald (1964) to have pointed out a fundamental difficulty with this postulated clear distinction between regular and sporadic changes: a number of the 'shining examples of regularity' belong to the allegedly sporadic class of changes, such as the Greek and Sanskrit aspirate dissimilations covered by Grassmann's Law.

Hoenigswald's tentative solution to this difficulty lies in considering the distinction 'regular' vs. 'sporadic' sound change invalid: ". . . rather than say that there are maverick processes in which sporadicity and distant conditioning are alarmingly combined, it could be, that distant conditioning produces the appearance of sporadicity, simply by leaving so much more scope for morpheme boundaries to intervene, and hence for analogy to operate."

3. In this latter conclusion, Hoenigswald's view probably will not find many adherents. Thus, when Lat. *fragrare* appears as both *flagrare* and *fraglare* in Vulgar Latin, or *cribrum* as *criblum* and *cribum* (Schopf 1919: 80–83 etc.), it is difficult to attribute these varying outcomes to analogy. Moreover, Togeby (1964) comes to the conclusion that of the many Romance dissimilations noted in Posner 1961, only three can be considered regular, all others being sporadic. Even of these three "regular" processes, two are of doubtful regularity: The dissimilation of *r* to *l* in the context V ___ V . . . [+ stop] *r*, while amazingly regular, fails to apply in *aratru* : Span. *arado*. And the explanation of the development of *meretrice* to *menetrice* (beside *meletrice*) seems somewhat far-fetched. The situation is not better for the dissimilation *r* > *l* in structures like *arbore* : Sp. *árbol, róbore* : *roble*. Among

the exceptions we find not only the possible 'cultismo' *martyre* : *mártir*, but also *priore* : *prior, sartore* : *sastre.*

At the same time, however, the fact remains that there are exemplars of the allegedly sporadic class of sound changes which are as regular as any of the typically regular changes. Hoenigswald's conclusion that the distinction between 'regular' and 'sporadic' changes is not as clear-cut or absolute as the neogrammarians would have it, therefore remains as an important challenge to historical linguists.

4. One possible way of meeting that challenge would be to establish that typically sporadic changes can apply in a regular fashion only under certain well-defined, special circumstances. This would permit retention of a basic distinction between regular and sporadic changes, while at the same time providing a principled account for the apparent exceptions to this distinction.

In this paper I will try to show that there is one area where it is possible to establish such special circumstances. This is the area of contact dissimilation, including the dissimilation of vowels in neighboring syllables (cf. the regularity of vowel assimilation in the same environment), but excluding dissimilatory loss.[1]

While this is a quite limited sub-area of the larger field of 'sporadic' changes, it is hoped that the findings of this paper, combined with Hoenigswald's challenge, will stimulate further research on the question of whether a principled account can be given for the apparently exceptional regularity of otherwise irregular processes.

5. In 1963, Benediktsson proposed that certain difficulties with the *u*-umlaut of *a* in Eastern dialects of Old Norse be explained as the result, not of two stages of umlaut (as had generally, but implausibly been assumed), but of a secondary "undoing" of *u*-umlaut in just those cases where the conditioning environment *u* had remained; cf. the chronology in (1). The explanation for this development, in his view, lies in the fact that before retained *u* there was a lack of contrast between original *a* and umlauted *ǫ*. (On the other hand, after apocope, *a* and *ǫ* had come to contrast elsewhere; cf. *all* vs. *ǫll* in (1).) The resulting archiphoneme then could be realized either as *a* (as in the Eastern dialects), or as *ǫ* (as in Old Icelandic), or (conceivably) as something in between.

(1)	pre-ON	allaz	allū	allum(–)	'all' (sg. N m., f., plD)
	umlaut		ǫllū	ǫllum(–)	
	apocope	all	ǫll		
	OIcel.	all	ǫll	ǫllum	
	OENorse	all	ǫll	allum	

6. Outside Germanicist/Scandinavianist cirles, Benediktsson's proposal does not seem to have found the attention it deserves. Consequently, when Schane wrote his famous 1971 paper in defence of the notion (though not a separate level) of the phoneme, he was apparently unaware of Benediktsson's paper. At the same time, he quoted and discussed a number of examples which are strikingly similar to the Old Norse "reversal" of *u*-umlaut.

Thus, in the history of French, vowel nasalization first was introduced before all nasals and later was eliminated where the conditioning nasal had remained and where thus there was no contrast between nasalized and non-nasalized vowels; cf. (2). Similarly, in Nupe, palat-

alization and labiovelarization are in the process of being eliminated in just those contexts where there is no contrast between palatalized (etc.) and non-palatalized (etc.) consonants. But before *a*, where there is a contrast, no change can be observed; cf. (3). Schane finds a similar situation in the case of Rumanian dispalatalization. Other parallel cases of dispalatalization can be added from many of the Slavic dialects (e.g. Ukrainian), from Lithuanian and Latvian, as well as from Old Irish, where however the process is limited to non-dentals; cf. (4).

(2)

pre-OF	bon	bonə
nasalization	bõn	bõnə
final N-deletion	bõ	
denasalization		bonə
attested forms	bõ	bon(ə)

(3)

Nupe	*eg^{y}i*	~ *egi*	'child'
	eg^{w}u	~ *egu*	'mud'
vs.	*eg^{y}a* 'blood' / *eg^{w}a* 'hand'	≠	*ega* 'stranger'

(4)

OIr.	*cnáim* [knāμ^{y}]	:	*cnám(a)i* [knāμi]	'bone(s)'
(vs.	*súil* [sūl^{y}]	:	*súili* [sūl^{y}i]	'eye(s)')

From the facts which he had examined, Schane concluded that (a) changes of this sort are limited to those positions where the relevant contrast is neutralized; (b) these developments involve the loss of a feature of "secondary" articulation (such as vowel nasalization or consonant palatalization); (c) under such conditions, changes of this sort are expected to occur.

7. Schane thus apparently independently has come to conclusions quite similar to Benediktsson's. However, in so far as they differ or go beyond the latter, they must be viewed with scepticism.

Thus the fact that Russian and many other languages have no dispalatalization before front vowel (in spite of a lack of contrast) and that Old Irish dispalatalization does not affect dentals, shows that changes of this sort are not as automatic as Schane claimed.

Moreover, in cases like the dialectal Old Norse change of *ǫ* to *a* it is difficult to conceive of the process as one which eliminates a secondary feature of articulation. The case is similar for Russian, where an early change of *o* to *e* after palatalized segments, leading to a neutralization of *o* and *e*, is followed by a dissimilatory backing of accented *e* to *o* in part of this neutralized environment; cf. (5). Likewise, in Modern Icelandic *n* goes to *d* before *n* in an environment in which *n* and *d* do not contrast;[2] and under similar conditions, *l* becomes *d* before *l*; cf. (6). But again, it is difficult to view these changes as eliminations of secondary articulation features.

(5) (i) Slav. o > e / [+ pal.] ___
**znajomŭ* > *znajemŭ* 'we know'

(ii) Ru. é > ó / [+ pal.] ___ [– pal.]
ber^{y}ém > *ber^{y}óm* (*"berëm"*) 'we will take'

(6) Icel. (i) n > d / VV ___ n^3
einn > [eidn] 'one'

(ii) l > d / ___ l
all > [adl] 'all'

8. Examples like (6) further show that the noted developments in neutralized environment are not just simple "reversals" of earlier changes. For the clusters *nn* and *ll* of Old Icelandic were not necessarily derived from earlier *dn* or *dl*. In fact, *nn* never reflects an earlier *dn*.

Rather, I want to argue that these changes are cases of regular contact dissimilation and that it is moreover precisely the condition of contrast neutralization between the input and the outcome of the change, common to all of the above examples, which is, if not a necessary, then at least a highly conducive condition for regular contact dissimilation in general.

A cursory examination of the data dealt with in examples (1) through (6) will show that all of the changes result in segments which are more dissimilar to the neighboring, conditioning segments than the earlier, unchanged sounds. Moreover, all of these developments are regular (with the possible exception of the Nupe change (3), which is still in progress). Finally, in all changes there is no contrast between input and outcome in the environment for the change. At the same time, outside these neutralized environments the changes do not take place.

Further examples of this sort, also regular, can be found elsewhere. Thus Pedersen (1909: 158 and 162, 170) notes that *nn* and *mm* change to *dn* and *bm* in Cornish. As far as I can tell, however, there are no contrasting structures with original *dn* and *bm*. Dieterich (1908: 81) observes a change of *ll* to *lt* in certain Modern Greek dialects. Original *lt*, however, had become *rt* in these and other Greek dialects; cf. Dieterich (1908: 65).[4]

In Sanskrit there are constraints of varying strictness against retroflex *ṣ* before *r* or another retroflex *ṣ* (cf. Wackernagel 1896: 232–234 with Debrunner's Nachträge 130–131). One of these seems to have resulted from regular dissimilation, namely that in (7) below, where *ṣ* > *s* / ___ *r*. (It is true, *r* is not retroflex, but alveolar. Phonologically, however, retroflex and alveolar behave alike in triggering *n*-retroflexion; cf. Hock 1979.) Here as elsewhere, the outcome of the change and its input did not contrast, due to the fact that in the environment after *i, u*, original *s* had gone to *š* > *ṣ* by RUKI. There is, to be sure, one exception, namely *ajuṣran*. But this form can be explained as due to leveling.

(7) (i) PIE *t(r)i-sr-es* 'three (fem.)'
PIIr. *tišras* (cf. Av. *tišrō*)
Skt. *tisras*

(ii) similarly *uṣar-* 'dawn' : *usras* (sg. G)
reduplic. *sarīsṛpa-* < **sarīṣṛpa-* (from *sṛp-* 'creep')
reduplic. *sisrate* < **si-ṣr-ate* (from *sṛ-* 'run')[5]
cf. reduplic. *si-ṣic-ur* (from *sic-* 'pour')

Also the change of *xs* to *ks* in most of the Germanic dialects (as in **uxs-* : OE *ox*, ON *oxe*, NHG *Ochse* [ks]) probably belongs here, with regular dissimilation of fricative *x* to *k* before fricatival *s*. True, Modern German has apparently unshifted forms like *machst* [xs] 'you (sg.) do'. However, these go back to forms like OHG *mahhōst*, with a vowel intervening between the *x* and the *s*. The modern [xs] can therefore be explained as due to the

fact that the change of *xs* to *ks* had taken place before the loss of intervening vowel in forms like *mahhōst*. (Alternatively, the [x] of *machst* can be explained as reintroduced by generalization from forms like OHG *mahhōt*, NHG *macht* 'does'.) Similar arguments are possible for forms like OE *æcs* 'axe' which seem to offer original *k* before *s*, suggesting that the contrast between *k* and *x* had not been neutralized before *s*. As the alternative Old English form *æces* and especially Go. *aqizi* show, *æcs* goes back to **ak(ʷ)iz/s-*, with a vowel intervening between *k* and the sibilant. The change *xs* > *ks* therefore may have predated the loss of that intervening vowel, at a time when *k* and *x* were not as yet in contrast before *s*.[6]

9. The data so far presented, then, strongly suggest that at least in the area of contact dissimilation, it is possible to discern a special condition which permits regular dissimilation, namely a lack of contrast between input and outcome in the environment of the change.

Evidently, in order to test whether this conclusion can stand as a universal condition for regular contact dissimilation, it would be necessary to examine all instances of regular contact dissimilation which ever occurred and to see whether they meet this condition. Negative results would then require that either we place certain limitations on this 'universal' condition or (if they are too common) that we add the condition to the large pile of 'junked' universals.

As it turns out, it is quite difficult to find genuine instances of regular dissimilation. Most textbooks, even articles and monographs exclusively dealing with dissimilation (such as Grammont 1895 or Schwyzer 1933) do not distinguish between regular and sporadic dissimilation. (The majority of listed cases, however, turn out to be sporadic.) Moreover, in many cases the history of a given language is not sufficiently well known to permit an unambiguous interpretation of apparently dissimilatory developments.

There are, to be sure, certain types of changes which are commonly enough attested as regular processes to make it appear that they can be used as evidence against our hypothesis. One of these is the common development of *tl* to *kl* (as in Lat. **pōtlo- > pōc(u)lum; vet(u)lu-* > PRom. **veklu-*; Lith. *eglė* 'fir' < **edlē*, cf. OPr. *addle*, Po. *jodła*). This change, with dental *t* becoming velar *k* before dental *l*, has been called dissimilatory (cf. e.g. Stang 1966: 107). And, though regular, this change has taken place in languages with a contrasting *k* before the *l* (cf. e.g. Lat. *oc(u)lu-* = PRom. **oklu-*). However, it is questionable whether *tl* > *kl* is simply a dissimilation. For there seems to be a cross-linguistic difficulty with sequences of non-sibilant dental + *l*, a difficulty which may be resolved in several different ways (beside the change *tl* > *kl*), such as anaptyxis (as in coll. Engl. [æθəliyt] 'athlete'), metathesis (as in OSpan. *dadlenos* > *daldenos, tidle* > *tilde*), or resyllabification (as in Mod. Span. *atlas* [at$las] vs. the ordinary syllabification of stop + *l* observed in *coplas* [ko$plas]). It is this cross-linguistic difficulty, rather than dissimilation, which seems to be responsible for the change *tl* > *kl*.

Another common process which has often been called dissimilatory (cf. e.g. Rohlfs 1950: 57–58, 65) is that of the type *kt, pt* > *xt, ft*, with change of stop to fricative before stop. At least in the case of Greek there is a chance that this change operated at a time when structures like *autos* had changed into *aftos* (via *avtos*), thus producing a contrast between *f* and *p* before *t*. However, fricativizations of this sort are also found before (fricatival) sibilants, as well as before other consonants. (Cf. also section 12 below.) They simply are instances of weakening. (Cf. Hock 1976.)

10. More specific potential counterevidence is found in the developments of Lat. **genmen > germen, *kanmen > carmen.* For the change of *n* to *r* before *m* looks like dissimilation; the change is regular (in so far as both items in which it could have applied show it); and there is a contrast of *n* with *r* before *m* (cf. e.g. *arma, sermō*). Note however that the number of examples (two) is so small that it becomes almost meaningless to speak of regularity. Moreover, the change has also been attributed to dissimilation with the later following *n* (cf. Leumann 1926:231), i.e. as a distant assimilation.

A more ample set of potential counterexamples consists of the apparently dissimilatory development of nasal *n* to non-nasal *r* after nasal *m* in Spanish (as in *hominem > (h)omne > (h)omre > hombre*). For as *humerum > (h)omro > hombro* shows, there was a contrasting sequence *mr.* Note however that the development of *hominem* to *hombre* is not simply the result of sound change. For *hombre* replaces earlier *(h)ueme.* Even the latter, however, probably is not directly inherited; for as *dominum > dueño* shows, inherited *-omin-* develops to *-ueñ-* just like original *-omn-* (as in *somnum > sueño*). Both *hombre* and *(h)ueme* therefore no doubt represent borrowings from (partly vernacularized) Medieval Latin. Grammont (1895: 138–139) therefore probably is correct in seeing in forms like *hom(b)re* nativizations of Med. Lat. *omne* (with vernacularizing syncope) into a language which had lost the sequence *mn* and which as a consequence used *mr* as the phonetically closest native substitute.

11. More serious is the evidence of Old English and Old Norse, where we find apparently regular dissimilatory developments of the type (8), in spite of the fact that the input to the change contrasted with its output. (In the following I draw heavily on data from Luick 1914 and Heusler 1931.)

(8) (i) OE *cȳsiþ > (*)cȳsþ > cȳst* 'thou choosest'
later OE *þiefþ > þieft* 'theft'
(ge)sihþ > gesiht 'sight'
cf. *wāst* 'thou knowest'
hæft 'prisoner'
þōhte 'thought'

(ii) ON **reisiðē > *reisþe > reiste* 'erected'
cf. *veist* 'thou knowest'

As it turns out, however, the developments in question are not always regular. Thus later Middle English has forms like *sucþ/icþ* for OE *sihþ* 'sees', with dissimilation of the first, rather than the second fricative. Moreover, the changes in (8) are associated with other processes which it would be hard to motivate as dissimilatory, cf. e.g. (9):

(9) (i) OE **auþmōdiþō > *ēaðmedþ(–) > ēaðmett-* 'humility'

(ii) ON **sakiþō > (*)sekþ > sext* 'guilt'
**diupiþō > *dȳpþ > dȳft* 'depth'
**wōhsliðe > *ϕkslðe > ϕkslte* ("*ϕxlte*") 'grew'
**halβiðō > helfð* [vð] beside dial. *helfþ/t* 'half'

Though the details of these developments are too complex to be traced in this paper, it may be suggested that the changes in (8) – as well as in ME *sucþ/icþ*, or in (9) – are not simple dissimilations. Rather, they seem to have been responses to certain structural con-

straints brought about by earlier changes: Grimm's Law, in combination with other developments, had led to a situation in which fricative-initial affixes such as the preterit marker *-ðē* alternated with stop-initial forms, as in (**nazjan-* 'save' :) **nazi-ðē* beside the rarer type (**þankjan-* 'think' :) **þăx-tē*. Synchronically, this alternation would seem to be best accounted for by a rule changing fricatives to stops after fricatives. (This is of course virtually the opposite of the historical change, by which voiceless stops had changed to fricatives, except after obstruent.) To this was later added the change of *xs* to *ks*. From both of these structural facts it was then possible to reinterpret or infer a more general constraint against double fricatives or (for Old Norse) a canonical form fricative plus stop for all (voiceless) obstruent sequences (except those of the type stop + sibilant); etc. In some cases (such as OE *gesiht* vs. ME *sucþ/zicþ*) the constraint might be implemented in conflicting ways. Finally, for forms like ON *þkslte*, even more complicated generalizations seem to be involved.

12. The potentially most damaging counterevidence against the hypothesis of this paper comes from the history of Greek: As is well known, not only did *kt, pt* become *xt, ft* in Modern Greek, the latter merging with the outcome of *aut, eut* > *aft, eft*; also *khth, phth* changed to *xt, ft*; and *sth, skh*, and to some extent *sph* became *st, sk*, and *sp*. As is also known, outside these clusters, the old voiceless aspirates *kh, th, ph* became the fricatives *x, θ, f*. It is the relationship between these developments which makes for potential challenges to the hypothesis of this paper.

Now, as noted in section 9, the change of *kt, pt* to *xt, ft* probably is not dissimilatory, but involves weakening. In fact, early Naxian *NaHsios* (with hēta), and the development of *ks, ps* to *ss* in Attic Vase inscriptions and elsewhere suggests weakening of stops to fricatives also before fricative, at least in certain varieties of ancient Greek. The Modern Greek of Southern Italy shows a similar fricativization even before *n* (Rohlfs 1950: 57, 64), and traces of such a change can be observed elsewhere in Modern Greek, as in Lesb. *áθna*, Comm. Gk. *áxna*, Pont. *áfna/áxna*, ultimately derived from Cl.Gk. *atmízō* (Hatzidakis 1892: 95). Weakening thus may once have had a very broad scope in the history of Greek. If outside of Southern Italy the effects of weakening appear only sporadically before nasal, this may reflect the diglossic influence of the archaizing standard language, just as in the case of the usual reflex *sf* for ancient *sph*. (The latter is almost certain to be explained in this fashion. For *sp*, sometimes beside hypercorrect *sf* for old *sp*, is found all along the periphery of Modern Greek, from Pontic in the East (Hatzidakis 1892: 162) via Cypriot and other Southern dialects (Dieterich 1908: 79–80) to Italian Greek in the West (Rohlfs 1950: 65, 72–73).)

There is however a potential problem in the fact that *khth, phth* merged with the outcome of *kt, pt*, i.e. with *xt, ft*, and that *sth, skh (sph)* became, and merged with, *st, sk (sp)*. If now it is assumed that these changes took place as in (10), with dissimilation in the fricative clusters created by the general fricativization of the old aspirates, then we might be confronted with a case of regular dissimilation, even though the input and the outcome of the change were in contrast:

(10)		*oktō*	*khthes*	*esthai*	*steira*
	Weakening	*oxto*			
	Fricativ.		*xθes*	*esθe*	
	Dissimil.		*xtes*	*este*	
	Attested	*oxto*	*xtes*	*este*	*stira*
		'eight'	'yester-day'	'be'	'sterile'

However, the exact chronology – and nature – of the changes is difficult to establish. At least one author, Dieterich (1898: 96–101), has on the basis of inscriptional evidence argued for a very different scenario, which can be restated as in (11).[7] In this scenario, the deaspiration after fricative would be typologically comparable to the absence of aspiration in English and German voiceless stops after tautosyllabic fricative.

(11)		*oktō*	*khthes*	*esthai*	*steira*
	Deasp. I[7]			*estai*	
	Weakening	*oxto*	*xthes*		
	Deasp. II[7]		*xtes*		
	Attested	*oxto*	*xtes*	*este*	*stira*

In this scenario, two processes of Deaspiration are invoked, so as to account for the sequencing of events as stated by Dieterich, who finds evidence for *sth* > *st* to be completed by the third century B.C., but for *kt/khth* > *xt* only from the fifth century A.D. However, Schwyzer (1939: 210–211) cites 'ἔκχταν (with unaspirated, non-fricatival *t*) from a fourth-century B.C. inscription from Cos. If this could be taken to indicate an early date for *kt/khth* > *xt*, then the two Deaspirations could be collapsed and could be ordered after Weakening.[8]

Given this possible alternative interpretation and the uncertainty of interpretation of the epigraphical evidence, it seems unnecessary to see in the above Greek developments counterevidence to the hypothesis of this paper.

There is, however, evidence that just as in Old English and Old Norse, the patterns and alternations created by the above changes (cf. e.g. Mod. Gk. *ap(o) to* → *af to*) made possible a reinterpretation according to which there is a general constraint against (voiceless) stop clusters or (voiceless) fricative clusters. And just as in Old English and Old Norse, this reinterpretation seems to have spawned off a number of developments, some of them dissimulatory, some not; some regular, some not; some in conformity with the conditions for regular dissimilation postulated in this paper, some not. Thus *xs, fs* have become *ks, ps* in almost all of Modern Greek, with the exception of Italian Greek (Rohlfs 1950: 63–64, 74–75); *ss* has changed to *ts* in a number of Greek dialects, some of them having *ts* from other sources (Dieterich 1908: 80), others apparently not (Pernot 1907: 286–291). The most extensive documentation of these changes (including such developments as *rθ* > *rt*, *rx* > *rk*, *γð* > *γt*, *γr* > *kr*) is found in Pernot (1907) who also points out that in terms of geographical distribution, many of these changes must be considerably later than *kt/khth* > *xt* and *sth* > *st*. Moreover, many of the changes which he cites are incomplete or irregular. For these and other reasons, his conclusion that all Modern Greek consonant clusters tend to become clusters of voiceless fricative + voiceless stop is only tentative.

13. While the evidence discussed in the preceding two sections thus does not provide cogent counterarguments to the hypothesis of this paper, it may require the following slight modification: The reinterpretation of certain phonological structures as exhibiting a contraint against particular segment combinations may lead to response changes which may include regular contact dissimilation, even in cases where the input and outcome of the change are in contrast with each other. At the same time, however, they may include other changes which cannot be easily viewed as dissimilatory.

Except for this possible modification of our hypothesis, however, the discussion in sections 9–12 has failed to turn up any cogent counterarguments, initial appearances to the contrary notwithstanding. Combined with the positive evidence presented in sections 5–8, this fact would seem to strengthen the case for the claim that regular contact dissimilation requires the absence of contrast between input and outcome in the environment of the change.

14. The most striking evidence for the validity of this hypothesis seems to come from the development of geminate sibilants in the prehistory of Sanskrit. For here the regularity vs. irregularity of a contact dissimilation seems to be directly attributable to the absence or presence of contrast, respectively, between input and outcome of the change. (The data, to be sure, are prehistoric, postulated intermediate stages in the development from Proto-Indo-European to Sanskrit. However, I believe to have shown elsewhere (Hock 1974, 1975) that they constitute plausible and necessary steps in this development.)

Disregarding an archaic degemination, by which double sibilant *ss* became *s* and which survives only in a few relics, we can note that Proto-Indo-Iranian geminate sibilants underwent regular contact dissimilation in the case of *šš* (> pre-Skt. retrofl. *ṣṣ*) and *śś*, but failed to do so in the case of *ss;* cf. (12). The reason for this different behavior can be sought in the fact that *ss* was in contrast with *ts* (cf. (12), while there was no comparable contrast between *ṭṣ* and *ṣṣ* or *cś* and *śś*. While the latter sibilants therefore were free to undergo regular contact dissimilation, in the case of *ss*, the dissimilation took place only sporadically (cf. *apassu* vs. *vatsyati*).

(12)

PIE	wiḱs	wiḱsu	-sḱe/o-	opessu	wessyeti	weytsyeti
PIIr.I	wišs	wišsu	-sśa-			
PIIr.II	višš	viššu	-śśa-	apassu	vassyati	vaitsyati
pre-Skt.	viṣṣ	viṣṣu				
Dissim.	viṭṣ	viṭṣu	-cśa-	— — —	vatsyati	
Attested	viṭ	vikṣu	-ccha-	apassu	vatsyati	vetsyati
	'clan' (sg.N)	'clan' (pl.L)	pres. suffix	'active' (pl.L)	desid. → fut. of *vas-*	desid. → fut. of *vid-*

15. Given the evidence presented in this paper, it seems safe to conclude that Hoenigswald's challenge concerning the neogrammarian distinction between regular and sporadic sound change can be met in the area of contact dissimilation: With the possible exception noted in section 13, it can be stated that contact dissimilation can become regular only in those cases where the input to the process and its outcome are not in contrast in the environment for the change.

Notes

[1] I also exclude diphthongization, a process which could be conceived of as dissimilatory, but for which many other explanations are possible; cf. e.g. the discussion and references in Wanner 1975.

[2] Modern Icelandic, to be sure, offers a few forms like *buddna* [büdna] 'of purses'. However, these seem to be recent borrowings. Moreover, if inherited, the [dn] of these forms can only reflect earlier *ddn*, not simple *dn*.

[3] A systematic exception consists of forms with suffixed definite article, such as *ey-nni* (sg.D f.) 'to the island'. These can be explained either in terms of relative chronology or as analogical: Forms like *ævi-nni* (sg.D.f.) 'to the life' suggest that *-nni* at one point was preceded by short *i*, which would have blocked *nn* > *dn*. Alternatively, the *-nni* of *ey-nni* can be viewed as analogical to the regular *-nni* of forms like *ævi-nni*.

[4] Also initial *l* may change to *lt*. This may either be attributed to external sandhi (Schwyzer 1933) or to the wide-spread gemination of initial consonants in Southern Greek (cf. Dieterich 1908: 61–62).

[5] Forms like *sisarti* 'runs', with *s* > *s* before non-neighboring *r* may either be explained as generalizations from *sisrate* etc. or as instances of the less regular constraints against *s* before distant *s* or *r*.

[6] Perhaps the Germanic change of *mn* to *vn*, as in **stemnō* : Go. *stibna* 'voice', ON *stefn* 'summons', belongs here, too. However, some of the details are difficult to establish and opinions on the interpretation vary. (Thus in 1897: 383, Brugmann considered the change regular, while in 1903: 115, he stated that the situation is 'unklar', and Prokosch (1938:87) considers the change sporadic.) While the change appears to be quite regular for Gothic and Old Norse, there is room for the suspicion that *mn* in some cases became *mm*, rather than *vn*; cf. e.g. OHG *stamm*, OE *stem(m)* 'stem', ON *stemma* 'to stop, stem' vs. OS *stamn*, OE *stefn*, ON *stafn* 'prow, stem' (cf. e.g. Brugmann 1903: 115).

[7] I have retained my weakening explanation for the preconsonantal stops, rather than subscribing to the quite speculative and complex hypothesis of Dieterich according to which the change progressed as follows: *pt* > *pht* > *phth* > *pfth* > *pft* > *ft*. As a consequence I have to invoke a second deaspiration process.

[8] The difficulties with interpreting the epigraphic evidence can be illustrated by the fact that from the second century B.C. onward we find both forms like καταπθιμενος (Chios) and like the ἐκχταν quoted above. Schwyzer (ibid.), taking forms with *πθ* at their face value, sees in the former an intermediate stage *pth*. However, *κχτ* suggests much earlier structures of the type *kt* or *xt*. (Dieterich takes them to represent *kxt*.) Perhaps the contradictory evidence is best explained as reflecting neutralization in the vernacular language of the earlier aspirate and unaspirated stops of these clusters, combined with attempts in the more archaizing educated language to retain the distinction in question. (Cf. the discussion in Sturtevant (1940: 83–85) on the retention of aspirate pronunciation in archaizing, educated Byzantine Greek, long after the vernacular change of these aspirates into fricatives.)

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Linguistic areas: getting at the grain of history*

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A perennial problem in the study of linguistic areas is the indeterminate nature of the data adduced to link observed effect with asserted cause. The present paper reviews four possible sources of structural parallels in adjacent languages: viz, coincidence, typological harmonics, genetic relationship, and cultural contact. The kind of data which select cultural contact as cause (and which exclude the three other possibilities) is identified and instantiated from field work in South Asia.

Prolonged periods of interaction among speakers of languages belonging to different (or at least not demonstrably related) families may lead to convergence of structure (or of other characteristics: see Hook 1982) which is not explainable except by reference to that interaction. This opens up the attractive possibility that we may use the study of linguistic areas (together with other kinds of tacit evidence) as means to inferring prehistorical cultural contact among such groups. As an instance of such inference (controlled by independent historical testimony), the absence of an infinitive in British Romani may be cited:[1]

kam -av-as te ja-v kher-e want-1S-IMPRF that go-1S home-LOC	(Sampson 1926:IV–131) 'I want to go home.'
brišindō ja-la te de -l rain go-3S that give-3S	(Sampson 1926:III–191) 'It's going to rain.'

This syntactic peculiarity is an indication of protracted residence in the Balkans. We can be fairly sure of this interpretation since all other forms of Indo-Aryan have an infinitive; the lack of one distinguishes Balkan languages from their neighbors;[2] and there are historical records of the Gypsies' sojourn there.[3] In this paper I attempt a preliminary discussion of a basis for the evaluation of convergence phenomena as indicators of cultural contact when there is no explicit historical record.

In his landmark study of South Asia as a linguistic area, Colin Masica (1976) makes an important methodological point: In a linguistic area the parameters along which languages of different families are similar must in fact define that area. That is, to establish a given geographical region as a linguistic area we must show that its languages have features in common which distinguish them as a group from languages outside the area. Thus, for each feature that he discusses, Masica traces its presence (or absence) in ever widening circles until he reaches the line that separates languages having it from those that do not. By following this procedure, Masica discovers a very surprising thing, surprising at least to most Indologists: The syntactic (and semantic) features he selects do not define South Asia as a linguistic area. Rather they show a commonality in structure that includes language spoken

in vast areas of the Soviet Union (the Central Asian Socialist Republics), Korea, and Japan, an area which Masica has more recently[4] termed the 'Indo-Turanian' linguistic area.

What are we to make of this? Linguists (at least those linguists who are Indologists) are familiar with the idea that Indo-Aryan, Dravidian and Mundan languages may have come to resemble one another as much as they do by virtue of the millennia of close cultural contact for which there is direct historical evidence as well as obvious manifestations in every sphere of life: religion, philosophy, music, cuisine, etc.[5] Are the historical incursions of White Huns and Mughals into India of the same order of magnitude and intensity to explain a degree of linguistic convergence between South Asian and Central Asian languages not much different from that found between Indo-Aryan, Dravidian and Munda in South Asia itself?

In fact, a great deal of attention need not be paid to this question for the same degree of convergence (or similarity) can be found between the Altaic of the seventh or eighth centuries A.D. (see Appendix A) and inscriptional Tamil of the same or even earlier periods (Appendix B). Considering the enormous distance separating the site of the Orkhon inscriptions from Tamilnadu and the absence of historical records of contact between Altaic and Dravidian speakers prior to the sixth century, we must either assume prehistoric contact (or very remote genetic relationship) to be the explanation or consider whether linguistic areas may owe their existence to mechanisms other than these. It is the nature of such 'other mechanisms' that must be understood before we know how to assess the historiographical importance of the Indo-Turanian linguistic area.

There are two explanations for linguistic areas which do not appeal to contact or to remote genetic relationship: (1) coincidence, and (2) typological harmonics. The likelihood of simple coincidence's being the explanation cannot be determined without a better understanding of typological harmonics. That is, unless we know, in general which typological features pattern (or tend to pattern) with which, we cannot judge what the probabilities are of a given constellation of features arising purely by chance. For instance, if we assume that the four features (out of the five used in Masica 1976[6]) that show linguistic parallels between South and Central Asia are both independent and roughly equipollent (i.e., equally likely to be present or absent), then the likelihood that a Central Asian linguistic area causally disconnected from South Asia would show the existing convergence simply by chance is one in sixteen. If some or all of those four features tend to pattern together in the world's languages then this rather remote probability of accidental convergence improves. And improves rather markedly. How else are we to explain the presence of languages of typically Indo-Turanian structure in the western cordilleras of South America (at the very antipodes of South Asia)? Ecuadoran Quichua, for instance, shares all the syntactic and semantic features with South Asian languages that Orkhon Turkic does (see Appendix C).

The work of Greenberg and others has shown that certain typological features tend to cluster together. If a language has a basic word order in which the object precedes the verb then it is much more likely to have postpositions than it is to have prepositions, auxiliaries are much more likely to follow their verbs than to precede them, etc. Thus, agreement among languages along several of these parameters cannot be considered cumulative evidence for establishing a linguistic area, or, to put the matter more precisely, such agreements cannot be considered additive. Rather, a complex formula has to be worked out

specifying the weight to be given to each individual parallelism. Recognition of this principle is implicit in the organization of Masica's discussion of South Asia as a linguistic area where word order phenomena are treated together in a single chapter, as is the 'dative subject' construction with the absence of a verb 'to have'. To work out such a formula will require that we determine ratios of incidence for each feature of interest to us in the total stock of the world's languages (or in some controlled sample of them: see Tomlin 1979) and then, assuming them to be independent, compute the expected frequency of each combination of them. Comparing these expected frequencies with those actually observed will tell us how much to discount for typological harmonics, even if there is no satisfactory explanation for those harmonics in linguistic theories presently available to us.

Let us take a very simple example of this: In Appendix II of his pioneering essay "Some Universals of Grammar with Particular Reference to the Order of Meaningful Elements", Greenberg classifies a largish number of languages according to different combinations of basic word-order, post- vs. prepositions, adjective before noun vs. noun before adjective, etc. The listing of languages and groups of languages he surveys totals approximately thirty-three lines of type. Taking three of these variables individually we find the following ratios of incidence:

A.	SOV word order	13	39%	SVO word order	14	42%
B.	postpositions	17	52%	prepositions	16	48%
C.	adjective-noun	12	36%	noun-adjective	21	64%

(Here the integers are the number of lines of type. They do not total 33 for variable A because I am not considering verb-first word-order here.) Assuming these variables to be independent of one another, we can calculate expected ratios of incidence for each combination. For simplicity's sake I will find the values for A into B and for A into C (the integer following the figure for percentage is the expected number of lines):

I.	AB	20%	7,	-AB	22%	7,	A-B	19%	6,	-A-B	20%	7
II.	AC	14%	5,	-AC	15%	5,	A-C	25%	8,	-A-C	27%	9

The observed percentages and numbers of lines for each of these combinations are:

I.	AB	36%	12,	-AB	12%	4,	A-B	3%	1,	-A-B	27%	9
II.	AC	18%	6,	-AC	12%	4,	A-C	21%	7,	-A-C	27%	9

Comparison of these two sets of figures shows that the 'basic word order' of a language is much more closely tied to post- vs. preposition than it is to the relative order of adjective and noun. That is, features A and B tend either both to be present or both to be absent to a stronger degree than do features A and C. Therefore, agreement on the latter between languages is more probative in the demonstration of a linguistic area than is post- vs. preposition and accordingly should be given a greater weight in the calculation (assuming that basic word order has already been given a full vote).

However, there is a limitation on this procedure which may be severe enough to dissuade anyone from undertaking the arduous task of determining separate frequency ratios for a large sampling of syntactic and semantic traits: It is possible that the world is not big enough, that the number of coexisting human languages is too small to give a representative sample of the full potential of man's language faculty and that cultural contacts are too frequent for us ever to know what the inherent typological characteristics of human language

would be if individual languages were allowed to develop undisturbed in pristine isolation. In short, all languages may belong to a single linguistic area, namely, the Earth.

Such 'bons mots' aside, there is another reason for not undertaking mammoth typological studies of the world's total stock of languages (interesting as such studies might be in themselves): From the point of view of demonstrating linguistic areas they are simply not necessary. Rather if we turn to detailed cross-dialect, fine-grain comparisons we can show a pattern of convergence that will conclusively put to rest the two-headed bogey of coincidence and typological harmonics and which will also distinguish contact phenomena from common retentions associated with remote genetic relationships (eg. Nostratic). If a statistical study of a given feature of syntax or semantics shows a regular correlation with distance, and, especially, if this correlation is independent of genetic groupings and subgroupings, then we have a prima-facie case for contact as cause.

Recently I have conducted field research on the geographical distribution of aspectual contrasts in South Asian languages. I happened to include a pair of complex sentences on the questionnaire which show a regular progression in the frequency ratios of subordinate-main as opposed to main-subordinate clause orders as one proceeds from north to south within western Indo-Aryan (Sindhi, Kutchi, Gujarati, Marathi). (See figure.) Subordinate-main order is typical of Dravidian; main-subordinate, of Persian. Since we are no longer dealing with a simple binary (yes-no, present-absent) value for a feature, but with a finely modulated correlation of frequency with distance, coincidence is ruled out.[7] Ascribing the facts to typological harmonics simply puts the operative factor at one or two removes and still does not explain the regularity in the geographic distribution of that factor. Similarly remote genetically based groupings and sub-groupings do not vary continuously with distance: i.e., the Stammbaum model would not predict that Marathi would share more syntactic and semantic features with Kannada and Tamil than the North Dravidian language Brahui does.

In effect, fine-grain correlations of this kind can be explained only by positing chains of bi-dialectal and/or bi-lingual speakers along which change in some feature of syntax or semantics is transmitted much in the manner distortions in a message are propagated and elaborated in the party game 'Telephone'. Such correlations promise to do for convergence studies what the regularity of sound correspondences did for the development of historical-comparative linguistics. The difficulties in establishing such correlations have to do with the availability of relevant information.[8] Before Labov linguists were rarely interested in gathering data on the relative frequency of competing forms.

Assuming that such data become available in the future, we may discover that the fine-grain processes and social interacitons that lead to clines in the frequencies of competing linguistic forms and structures may be relatively short-lived while their global effects may persist for centuries after them. For example, the Indo-Turanian linguistic area uncovered by Masica may be a kind of fossil, a trace of an area once knit together by chains of bi-dialectal and bilingual speakers, but now subject, in its various parts (and from diverse directions), to new influences that have yet to obscure the large-scale parallels in structure established in prehistoric times. An ambitious survey of competing patterns in a number of linguistic dimensions carried out on a village-to-village scale along selected geographic arcs in South Asia and Central Asia is needed to answer this question.

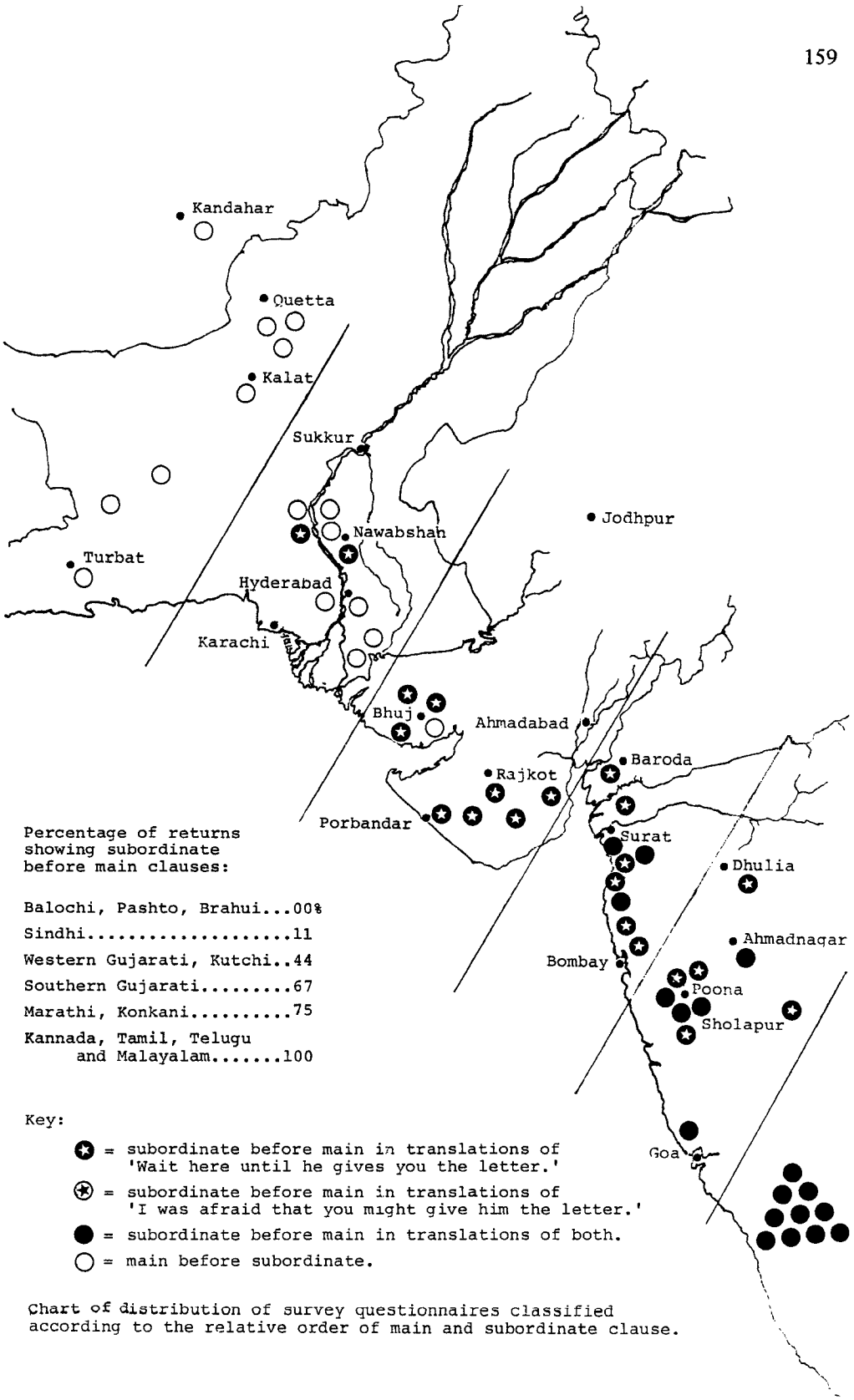

Chart of distribution of survey questionnaires classified according to the relative order of main and subordinate clause.

List of Abbreviations

ACC	accusative
Adj	adjective
CAUS	causative
CIS	cislocative
CP	conjunctive participle
DAT	dative
EMPH	emphatic
ERG	ergative
GEN	genitive
HORT	hortative
IMPER	imperative
IMPRF	imperfect
INF	infinitive
INSTRU	instrumental
LOC	locative
N	noun
NP	noun phrase
OBL	oblique
Pl	plural
POSS	possessor
PRES	present
PST	past
PSTP	past participle
RelP	relative phrase
S	singular
Sg	singular
SM	standard-marker
SOV	subject-object-verb
T	tonral
1	first person
2	second person
3	third person

Notes

* This paper derives from one presented at the thirty-fifth annual meeting of the Association for Asian Studies (San Francisco, 1983) as part of a panel on South Asia as a linguistic area. The field work on which it is based was carried out in India and Pakistan starting in 1978 on research trips supported in varying degrees by the Horace H. Rackham School of Graduate Studies, University of Michigan; the American Institute of Indian Studies; and the United States Educational Foundations of India and Pakistan (Fulbright-Hays Faculty Research Abroad). Their financial and organizational assistance is gratefully acknowledged as is the help and co-operation of colleagues at the University of Delhi, the Central Hindi Institute (Agra), the Northern Regional Language Centre (Patiala), the University of South Gujarat (Surat), the Oriental College at the Unversity of the Panjab (Lahore), the Institute of Sindhology (Jamshoro), the Pakistan Information Department and many other institutions and individuals in both countries. I am particularly indebted to Mrs. Kathleen Chaudhry of Lahore for her constant help, introductions and hospitality and to Dr. H.K. Gaur of Surat for providing much needed data from Western Gujarat.

[1] The first South Asians to reach the shores of England arrived well before the East India Company was founded, at the latest by 1500 according to Vesey-Fitzgerald (1944:20). Their northwesterly migration began sometime before 250 B.C. (Turner 1927:23)

[2] See Sandfeld (1930) and Solta (1980). The Balkan languages that lack the infinitive include:

Modern Greek:	*θελο να πα-ω* I-want that go-1Sg	'I want to go.'
Bulgarian:	*toj ne iska da dojd-e* he not want that come-3Sg	'Il ne veut pas venir.' (Beaulieux 1950:326)
Rumanian:	*vreau sa cînt* I-want that I-sing	'Je veux chanter.' (Cazacu 1967:125)
Albanian:	*dua të shkruaj* I-want that I-write	'I want to write.' (Solta 1980:212)
Macedonian:	*saka da ja rešava* I-want that it I-solve	'He wants to solve it.' (Lunt 1952:85)
Grecanica-Bova: Calabrese:	*íθel-a na mu fer -í* *vuli-a mu mi pòrt -a* he-wanted that me bring-3SG	'He'd like to bring me.' (Solta 1980:214)

[3] For an Arab report of Gypsies among the Byzantines in the ninth century A.D. see Vesey-Fitzgerald (1944:6ff.).

[4] In his paper entitled "South Asian languages: typological coincidence or areal convergence?" presented at the 35th annual meeting of the Association for Asian Studies, 1983.

[5] For a first look at such convergence in folk narratives, see Hook (1979).

[6] The fifth one Masica discusses, the 'dative subject', does not exist in Central Asian languages.

[7] The probability of coincidence's being the 'cause' in the appearance of a regular progression in a series of geographically ordered data sets is equal to $\frac{2}{n!}$ where n is the number of sets. In this particular instance, where there are six such sets, the chances of their exhibiting such a progression purely by coincidence are two in 6 x 5 x 4 x 3 x 2 x 1 or one in three hundred sixty (0.0028).

[8] Relevant information has sometimes been collected, by accident as it were. For an attempt to extract such information from Grierson's linguistic survey of India, see Hook (1977).

[9] Part of Deshpande's argument for a Dravidian source for retroflexion in the Ṛgveda appeals to geographical information (1979:252ff). Southworth 1974 goes considerably further in his use of data from Grierson to infer, on the basis of the incidence of retroflexion in modern Indo-Aryan, that Maharashtra, Gujarat and Sind are the areas most likely to have been Dravidian-speaking at the time of the Aryan incursions into South Asia. The problem with this is that the present distribution of linguistic features may have nothing to do with the situation that obtained three to four thousand years ago. For an analogous difficulty in attributing the *gorgia toscana* to an Etruscan substratum see Kurath (1972:154–156).

Appendix A: Orkhon Turkic

Orkhon Turkic, as known to us from inscriptions first discovered 250 years ago in what is today central Mongolia, shares with Dravidian the four syntactic and semantic parallelisms that Masica 1976 uses to define the Indo-Turanian linguistic area: (1) word order, (2) derived causative verbs, (3) conjunctive participles and (4) explicatior compound verbs. Examples of these (unless otherwise indicated, from the eastern or southern faces of the Kül Tigin inscription) are:

(1) word order:

a *tutuq -uγ älig -in tut-di* (E 38) SOV
governor-ACC hand-INSTRU captured
'(He) captured the governor by (his own) hand.'

b *türük qara qamaγ bodun* (E 8) AdjN
Turk black entire people
'. . . the Turkish common people. . .'

c *ini -m kül tigin birlä* (E 26) Postposition
brother-my Kul prince with
'. . . with my brother, Prince Kul. . .

d *ötükän yiš -da yig idi yoq ärmis* (S 4) SMAdj
Otukan mtn-from better at-all not is
'There is no better (place) than the O. Mountains.'

e *umay tägög -üm* (E 31) RelP-NP
Umay like mother-my
'My mother (who is) like the Goddess Umay. . .'

(2) derived causatives (and transitives):

a *öl-* 'die' *ölür-* 'kill'; *käl-* 'come' *kälür-* 'bring'

b *är-* 'be' *ärtür-* 'make'; *yarat-* 'make' *yaratur-* 'have made'

c *aŋar adɨnč(č)ɨγ barq yarat-ur -t -um*
them (DAT) wonderful tomb build-CAUS-PST-1Sg
'I had them build an extraordinary mausoleum.'

(3) The affix of the conjunctive participle (CP) in Orkhon Turkic has a number of forms. Chief among these are a vowel (*-a/-ä, -ɨ/-i, -u/-ü*) and an ending *-p* or *-pan/-pän*. Like their counterparts in Dravidian (and unlike English *having V-ed*) the Orkhon conjunctive participles show great syntactic freedom occurring in imperatives and questions (as well as in the narrative declaratives where analogous constructions occur in English and most languages):

a *yaraqlɨ qantɨn käl -ip yań -a eltdi* (E 23)
armed whence come-CP disperse-CP sent
'From where did the armed (ones) come and scatter (you)?'

b *buni kör-ü bil -iŋ* (S 12)
this (ACC) see-CP know-IMPER
'See this (inscription) and learn (from it)!'

The same-agent constraint (Hook 1976) applies (although such agents do not have to be the same in surface structure):

c *yoq(q) aru at yät -ä yadaγin iγač tutun-u aγ -turt -um*
upwards horses lead-CP on-foot trees cling -CP climb-made-I
'I had them climb upwards on foot leading their horses and clinging to the trees.'
(Tonyukuk I, N 1)

(Here, it is clear from context that the leader of horses and the clinger to trees is not the 'I' of the account.)

(4) Explicator compound verbs in Orkhon Turkic, as in Dravidian, are formally indistinguishable from conjunctive participle plus finite verb constructions (cf. 3a):

a *uluγ irkin azqïna är -in täz -ip bar-d -i* (E 34)
great (name) few men-INSTRU flee-CP go -PST-3Sg
'The great Irkin ran away with a few (of his) men.'

The explicators are homophonous with lexical verbs:

b *bodun yer -in sub -in id -ip tabγać-γaru bardï*
people land-their water-their leave-CP China -toward went
'. . . people left their land and water and went toward China'.
(Bilgä Kagan incription, E 35)

c *qaγan-in yitür-ü id -mis* (E 7)
kagan-their lose -CP leave-PST
'. . . (they) lost their kagan. . .'

The alternation of the explicator with its absence (one of the two criteria for the classification 'compound verb'; see Hook 1977:336) is found in Orkhon Turkic under similar conditions:

d *kül tigin qoń yïl -qa yiti yegirmi-kä uć -dï* (KT NE)
Kul Prince Sheep year-LOC seven twenty -LOC fly-PST
'Prince Kul died in the Year of the Sheep on the 17th day.'

Here the tone is matter-of-fact. Contrast this with:

e *uč -a bar-d -ï-γ-ïz* (KT SE)
fly-CP go -PST-2 -P1

where the writer, Prince Kul's nephew, expresses his sense of loss (The sentence follows directly on: 'You used to nourish (the people) better than your beloved children and descendants.').

(5) Another feature common to Orkhon Turkic and Dravidian of the time is the use of participle of the verb 'say' (*te-/ti-*) as a complementizer with verbs of speaking and thinking and to introduce clauses of purpose (cf. Kuiper 1967):

a *olur-uγ ti -yin te -mis* '"Stay!" he said.'
stay-2PL say-ing say-PST (Tonyukuk I N 10)

b *üküs te -yin nä -kä qorq-ur biz* (Tonyukuk II W 4)
many say-ing what-DAT fear -PRS we
'Why should we be afraid (of there being) many (of them)?'

c *arqïš id -maz ti -yin sülä -d -im* (Bilgä Kagan E 25)
caravan send-not say-ing campaign-PST-1Sq
'I went to war (against them) because they did not send the (tribute) caravan.'

Appendix B: Inscriptional Tamil

The earliest Tamil prose of appreciable length is that found some fifty years ago in an inscription of some forty lines that was unearthed in Paḷḷaṅkōvil, a small village in Tanjore. The text, recording a grant of land, is considered to date from before 550 A.D. (Zvelebil 1964:6). From it we may illustrate the same syntactic and semantic features as have been exemplified in Appendix A.

(1) word order:

a *vajranandi-kkuravar-kk -e . . . nilaṅ koṭu-tt -om* (54–56) OV
(a name) teacher -DAT-EMPH land give -PST-we
'To the same teacher V. . . . we gave land. . .'

b *tan nāṭ . . .* (29) AdjN
your district. . .

c *peru -nāṅk-ellai -y -aka -ttu* (47–48) Postposition
great-four -limit-T-interior-OBL
'. . . within the great four boundaries. . .'

d (no comparatives available in the inscription)

e *kaṉṟu-mey pāl* (49) RelP-NP
calf -graze portion
'. . . the part (where) the calves graze. . .'

(2) derived causatives:

While no clear instances are available from the Paḷḷāṅ-kōvil inscription, they may be found in earlier inscriptions (from 200 B.C.) found in caves:

a *siri yakaru cānatāritāṉ koṭū-pi -t -oṉ* (Ariṭṭāpaṭṭi II, Zvelebil 1964:552)
shri (name) (name) cut -CAUS-PST-3Sg
'Shri Yakaru Canataritan . . . had (this cavern) cut.'

Compare this with the non-derived transitive in:

b *veḷaṭai nikāmātor koṭior* (Ariṭṭāpaṭṭi II, in Zvelebil 1964b:552)
(name) citizens (?) cut
'The people of Velatai cut (the cavern).'

(3) conjunctive participles:

a *nāṭṭār -um tirumukaṅ kaṇ-ṭu toḻu-tu. . .* (33–34)
members of nāṭu too royal-order see -CP namaskṛtya
'And the members of the district assembly having seen the royal proclamation and having shown their deference. . .'

(4) As in Orkhon Turkic the morphology of the explicator compound verb construction is indistinguishable from that of conjunctive participle plus finite verb:

a *aṟai -y -olai cey -tu koṭu-ttu viṭu -taka. . .* (32–33)
speak-T-leaf make-CP give -CP leave-HORT
'. . . prepare the proclamation and issue (it).'

Note the use here as in Orkhon Turkic of a verb homophonous with the verb for 'leave' as explicator.

(5) The conjunctive participle of the verb *eṉ* 'say' is used as a sentential complementizer (see Kuiper 1967):

a *tāṅkaḷ -um . . . aṟaiyolai cey -tu koṭu-ttu viṭu -taka -v -eṉ -ṟu*
you-PL-too speak-leaf make-CP give -CP leave-HORT-T-say-CP
nāṭṭār -kku t -tirumukam viṭ -a (31–33)
members-DAT T-royal-order leave-INF
'(The king) has given an order to the members of the assembly that they too should . . . prepare a proclamation and issue (it).'

Appendix C: Marathi and Quechua

Marathi is one of the Indo-Aryan languages most strongly Dravidianized in its syntactic and semantic structures. The examples adduced here show structural parallels between it and Quechua, the modern descendant of the state language of the pre-Columbian empire of the Incas. Convergence through cultural contact may in this instance be safely ruled out.

(1) word order:

a *mi tyā-lā paise -ϕ de -t -o* SOV
ñuka pay-man kullki -ta ku -ϕ -ni (Muysken 1977:19)
I him-to money-ACC give-PRES-1Sg
'I'll give the money to him.'

b *sundar stri* AdjN
sumak warmi
pretty woman

c *bābān-ϕ -čā mitrā -ϕ -či šeti -ϕ* Postposition
tayta -a -pa amiigu-m -pa čakra-n (Parker 1976:91)
father-POSS-GEN friend -POSS-GEN farm -POSS
'(my) father's friend's farm. . .'

d *mājhā-hun hušār* SMAdj
noca -manta amauta (Markham 1972:28)
me -from smart
'wiser than me. . .'

e *huān-ni di -lela patr -ϕ pāṭhav* RelP-NP
juan cu -shca quillca-ta cachai
Juan -ERG give-PSTP letter -ACC send
'Send the letter that Juan gave.'

Sentential objects, in both Marathi and Quechua, often follow their verbs (see sentence 5-a below).

(2) derived causatives (and transitives):

a *mar- mār- mār -av-*
wañu- wañu-či- wañu-či-či-
die kill kill -CAUS
('have someone kill')

(3) conjunctive participles:

a *ghar-ā -lā pohots-un hose ǰeva -lā*
wasi-ϕ -man chaya -shpa huzi miku-rka (Muysken 1977:29)
home-OBL-to arrive -CP Jose eat -PST
'(After) coming home Jose had his meal.'

(4) As in Altaic and in Dravidian, the explicator compound verb construction is based on the conjunctive participle form of the main verb in both Marathi and Quichua (Quichua is the Ecuadoran form of Quechua). Explicator constructions according to Bruce Mannheim (personal communication) are not to be found in Peruvian and Bolivian dialects of Quechua.

a *vāts -un de*
cati -shpa cuy (Vazquez 1940:126)
read-CP give
'Read (this) (for me).'

In the Sierra this construction has entered Spanish as a polite imperative or request:

b *da -me le -y -endo esta carta* (Toscano Mateus 1953:284)
give-me read-T-ing this letter
'Please read this letter for me.'

In Quichua the construction, apparently limited to transitive verbs, may include explicator auxiliaries homophonous with the verbs for 'give', 'leave', 'put' and 'throw'. The system appears more extensive in Marathi (see chart in Masica 1976:146).

(5) Both Marathi and Quechua use the conjunctive participle of a verb 'to say' as complementizer:

a *ma-lā kala-vi -ϕ -ϕ -la sukh-rup pohots-ϕ -lo mhaṇ-un*
ni -chi -mu-wa-rqa -n allinta chaya -mu-ni ni -shpa
me-to tell -CAUS-CIS-me-PST-3 safely arrive -CIS-1Sg say -CP
'He informed me that he had arrived safely.'

b *bagh-at āhe zhāḍ-ān -čāt pakši-ϕ pakaḍ-āy-tsāy mhaṇ-un*
riku -u -ni yura-una-ma pisku-ra api -nga-k ni -sha
look-ing-1Sg tree -PL -LOC bird -ACC grabbing -(for) say -CP
'I'm looking in the trees in order to catch a bird.'

(6) Marathi and Quechua both have a 'dative-subject' construction (although as Masica 1983 has noted, this is substantially less developed in Quechua):

a *ma -lā zhop-āytsa āh-e*
nuka-ta punu-naya -ϕ -n (Muysken 1977:123)
me -to sleep-want be-3Sg
'I feel like sleeping.'

This is a feature South Asian languages do not share with those of Central Asia.

(7) Emeneau (1974) shows that Sanskrit *api* and Dravidian *-um* have the same semantic range: 'also'; 'and' (in a series); 'even', 'although'; as a totalizer ('all six', 'both', etc.); and as an indefinitizer: *kopi* 'whoever' = *kaḥ* 'who' + *api* 'also'. Most of this can be shown for Quechua *pish*:

a *ge-l -o hi*
ri -rca -ni pish
go-PST-1Sg also
'(and I) went, too.'

b *kay-mam-pis wak -mam-pis* (I cannot find this pattern in Marathi.)
this way also that way also
'. . . this way and that. . .' (Parker 1976:146)

c *p'iña -ri -c -pi -pish mana ri -sha -chu* *rāg ye -un hi kāy upyog*
anger-go-er-on-also not go-will-no anger come-CP also what use
'Even if he gets angry, I won't go.' 'Even if you get angry, it's no use!'

d Quechua: *pi* 'who', *pi-pis* 'somebody'; *ima* 'what', *ima-pis* 'something'
Marathi: *koṇ* 'who', *koṇ-i* (*koṇ* + *hi*) 'somebody', *koṇi hi* 'anyone'
kuṭha 'where', *kuṭha hi* 'anywhere'
kāy 'what', *kāhi* 'some', 'something'

e *pāts-hi gele* (absent in Quechua?)
five -also went
'All five went.'

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Mantra glosses in the Śatapatha Brāhmaṇa: more light on the development of the Vedic verbal system

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The Śatapatha Brāhmaṇa is in great part a vast commentary on the mantras of the Vājasaneyi Saṃhitā (= White Yajurveda). As such, it often quotes a mantra directly and then glosses or paraphrases it, wholly or in part, sometimes with a gloss pure and simple, as in I.3.4.16:[1]

> *dhruvā́* asadann *íti dhruvā́ hy* ásadan
> 'Firm they have sat down', for firm they have sat down.

more often by describing the actions appropriate to the speaker of the mantra or the motivation for its use, as in I.1.3.11:

> *sá* prókṣati *agnáye tvā júṣṭaṃ* prókṣāmī́*ti*
> He sprinkles (saying) 'I sprinkle thee, enjoyable to Agni'.

As these examples show, the prose paraphrase is generally as close as possible to the wording of the mantra, with technical adjustments in person and the like, necessitated by the switch from the putative speaker of the mantra to the commentator on it. Novelty and variety of phrasing are not valued for themselves.

Given this faithfulness, it is in fact surprising how often the prose expression deviates from its text. These deviations are all the more interesting and valuable because they must result from pressures strong enough to overcome the counterpressure to repeat the mantra exactly. These discrepancies provide an almost ideal window on the development of the Vedic verbal system, and one that has been surprisingly neglected, as Renou pointed out almost 30 years ago (Histoire de la langue sanskrite (1956) 47, n.2). I know of only one brief treatment – that of Jayasuriya in the *Ceylon University Review* (1957). Though useful, it is hardly systematic, and, moreover, not easily accessible to many Western scholars.

This paper will examine the deviation in verbal forms, using a complete collection of the glossed verbs in the ŚB (Mādhyandina recension). At first glance, the data seem bewildering, almost intractable: the problem is that glossing practice is remarkably inconsistent. Obsolete forms are sometimes glossed by their modern replacements, sometimes left unglossed. So, *pā́ti* 'protects' is glossed by *gopāyáti* 'id.' 15 to 20 times (e.g. I.5.1.22, I.9.1.20), but appears in unglossed mantras about an equal number of times (e.g. I.8.3.19, I.9.2.17). Forms which remain at least marginally alive in prose are sometimes replaced when they appear in mantras, sometimes not, sometimes glossed by themselves. For example, *bhárati* is glossed by *hárati* 4 times (e.g. III.2.2.25, VI.6.3.8), but is commonly glossed by itself

(e.g. VI.4.4.8) or left unglossed (e.g. II.4.2.15). *Bhárati* is rarely used independently in prose, but does occasionally appear (e.g. I.4.2.2, where it was induced by vocative *bhārata*; VIII.1.1.9, induced by *bharádvāja-*). Indeed, of forms of the same morphological class, parallel to each other in the same passage, one may be modernized, the other not, as with the *s*-aorists in IX.2.3.20:

> yákṣad *agnír devó devā́m̃* ā́ *ca* vakṣad *íti* yákṣac *caivā́gnír devó devā́n* ā́ *ca* vahatv *íty etát*
> 'Agni the god shall worship the gods and convey (them) hither', that is 'Agni the god shall both worship the gods and let him convey (them) hither'.

The picture seems to be one of a verbal system in flux: where even obsolescent forms remain within the passive command of the speech community and can often still be used actively, though perhaps with self-conscious archaism.

However, despite these inconsistencies, certain patterns do emerge – patterns of two types. On the one hand, it is possible to assess the *relative* stability of categories within the verbal system, and of individual roots and stems. On the other, certain kinds of glossing practice cluster in certain portions of the text: in particular a large number of these practices are confined to, or far more common in, the Śāṇḍilya kāṇḍas VI–X, and absent, or almost absent, from the Yājñavalkya kāṇḍas. I will examine these two types of patterns in turn.

With regard to verbal categories, the general picture is consistent with the common opinion on the development of the Vedic verbal system; surprises may lie in the relative strengths of the various categories. Two categories of relative stability are voice and inflectional endings. Substitutions of voice are very rare. Of those that do occur, there are a very few apparently unmotivated substitutions, as in VII.2.2.5:

> *yunákta sī́rā* ví *yugā́* tanudhvam *íti yuñjánti hí sī́raṃ* ví *yugā́ni* tanvánti[2]

But most changes in voice are an automatic result of substituting a stem with one inherent voice for one with its opposite; for example, in VI.7.2.2 the active aorist *vy àdyaut* is replaced by medial present *vidyótate*, since the aorist is only active, the present only medial. A special case of this is the occasional replacement of the old athematic intensive with the newer *-yáte* type, as in VI.4.4.7, with part. *kánikradat* glossed by *kanikradyámānaḥ*.[3]

In inflectional endings, there is some tidying up of earlier alternative endings, e.g.

> VII.5.2.39 *śṛ̊ṇudhī́* → *śṛ̊ṇú*
> VI. 8.1.6 *juhotana* → *juhuta*
> VII.3.1.22 *bharāmasi* → *harāmas*

but these changes are rare. Irregular *nominal* endings seem more likely to be replaced (e.g. neut. pl. *-ā* by *-āni*).

More surprising is how rarely one alternative stem is substituted for another within one aspect – i.e. one type of present for another, one aorist for another – despite the plethora of multiform presents and aorists in Vedic. This happens occasionally in the aorist with short, ambiguous second and third person forms, which are converted into longer forms with clear personal endings:

I. 2.2.15 (= III. 9.4.18)	*bheḥ*	→ *bhaiṣīḥ*
VI. 3.3.14	*ákar*	→ *ákaraḥ*
VII. 1.1.43	*abhār*	→ *abhārṣīt*
XIV. 2.2.16	*ayāṭ*	→ *ayākṣīt*

In the present it hardly occurs except (1) in cases of phonological change – loss of occlusion in voiced aspirates (*bhárati* → *hárati* discussed above; *gr̥bhṇáti* → *gr̥hṇáti* 4times, e.g. VI. 4.4.17); (2) in cases involving *-áya-* formations, usually when the *-áya*-formation appears to constitute a new root (e.g. *śrīṇātu* → *śrapáyati* III. 8.3.20; *pr̥ṇáti* → *pūráyati* e.g. VI. 5.4.12). The major exception here is the root *kr̥*, whose old stem *kr̥ṇóti, kr̥ṇuté* is completely obsolete in the ŚB; *karóti, kuruté* has taken over its functions. In the glosses as well, *karóti* can replace *kr̥ṇóti* (e.g. I. 9.2.20 *kr̥ṇu* → *kuru*). Yet in fact *kr̥ṇóti* often remains unglossed in the mantras in which in appears: *karóti* glosses it only 7 times.

Far more common is the substitution of one aspect for another. Often this is semantically or situationally motivated: the description of the ritual requires an expression of present time, and the mantra contains a perfect or an aorist, e.g.

IV. 6.9.12 ávidāma *devā́n íti* vindánti *hí devā́n*
'We have found the gods', for they find the gods.

III. 6.4.14 *ayáṁ hí tvā svádhitis tétijānaḥ* praṇināya ... *íty eṣá hy ènaṁ svádhitis téjamānaḥ* prá ṇayati
'For this very sharp ax led thee forward' (he says), for this sharp ax leads him forward.

However, even when these pragmatically conditioned forms are left aside, substitution of aspect remains common. Most of these changes involve modal forms and participles, and the prose substitute is almost always a present. The trend in the language is to restrict non-indicative forms to the present stem; these data show how far this restriction has advanced in the ŚB, as Jayasuriya already pointed out. Compare the following examples, where no inherent semantic difference between the aspects can be found:

Imperative:

I.3.4.12 *ā́ tvā vásavo rudrā́ ādityā́ḥ* sadantv *íti* ... *eté tvā́*sīdantv *íty evaítád āha*
'Let the Vasus, Rudras, and Ādityas sit on thee', i.e. he says by this 'Let them sit on thee'.

IV. 1.1.22 *mánas tvāṣṭv íti prajā́patir vaí mánaḥ prajā́patiṣ ṭvāśnutām íty evaítád āha*
'Let mind reach thee.' Mind is Prajāpati. Thus he says by this 'Let Prajāpati reach thee'.

Subjunctive:

IX. 2.3.42 *yáthā devā́ ihā́gámann íti yáthā devā́ ihā́gáchān íty etád*
'So that the gods shall come hither', that is 'so that the gods shall come hither'.

Participle:

VI. 7.2.2. dr̥śā́ṇó *rukmá urvyā́ vy àdyaud íti* dr̥śyámāno *hy èṣá rukmá urvyā́ vidyótate*
'Appearing as an ornament, widely he has flashed forth', for appearing as an ornament, widely he flashes forth.

However, the case is strikingly different in indicative forms where no present-time expression is needed. All three past tenses (imperfect, aorist, and perfect) are relatively stable, and substitutions, when they occur, are chaotic, without a clear direction. Though aorists and perfects may be replaced by imperfects, the change also goes in the other direction, and aorists and perfects also interchange with each other. Following are examples of all of these possibilities:

Aorist → imperfect

VII. 5.2.36 *ajó hy àgnér* ájaniṣṭa *śókād íti yád vaí prajāpateḥ śókād* ájāyata *tád agnéḥ*
'For the goat was born from Agni's heat.' Truly, what was born from Prajāpati's heat, that (was born) from Agni's.

Perfect → imperfect

VI. 7.4.3 *divás pári prathamáṃ* jajñe *'gnír íti prāṇó vaí diváḥ prāṇád u vā́ eṣá prathamám* ajāyata
'Agni was born first from heaven.' Heaven is really breath, and he was indeed born first from breath.

Imperfect → perfect

V. 1.4.8 *gandharvā́ḥ saptáviṁśatis té 'gré 'śvam* ayuñjann *íti gandharvā́ ha vā́ agré 'śvaṃ* yuyujur
'The 27 Gandharvas yoked the horse at first.' Indeed the Gandharvas yoked the horse at first.

Imperfect → aorist

IV. 4.4.11 *yā́ṃ* ā́vahaḥ *uśató deva devā́n . . . íti . . . yā́n devā́n* ā́vākṣīḥ
'Which eager gods, o god, you conveyed hither . . .' i.e. 'which gods you have conveyed hither . . .'.

Aorist → perfect

VII. 5.1.9 *tríṁt samudrā́nt* sám asṛpat *svargā́n íti . . . tā́n eṣá . . .* anusáṁ sasarpa
'He crept over the 3 heavenly oceans' . . . he crept over them.

Perfect → aorist

IV. 4.4.10 *sugā́ vo devā́ḥ sádanā akarma yá* ājagmédáṁ *sávanaṃ juṣāṇā́ íti sugā́ni vo devā́ḥ sádanāny akarma yá* ā́gantedáṁ *sávanaṃ juṣāṇā́ íty evaítād āha*
'We have made pleasant seats for you, o gods, who came to this pressing, enjoying.' He says by this '. . . who have come . . .'.

As far as I can see, no narrowed semantic spheres have been established for the three different tenses; they remain in competition throughout their ranges. Even the aorist retains a large domain: recall the examples cited above of ambiguous aorist forms glossed by other aorists, not only injunctives in *mā́* prohibitives, but also augmented indicatives. And augmented aorists often gloss themselves. For examples *ásadan* in I. 3.4.16 quoted at the beginning of the paper, *ā́sadat* (VI. 4.4.17), *ā́sadas* (VI. 8.2.6); in contrast we just noted the aorist imperative *ásadantu* (I. 3.4.12), close to the aforementioned *ásadan*, replaced by a present imperative.

In mood, clear directions can again be perceived. Though not particularly common, substitutions in mood are almost always explicable, serving to set up or reinforce semantic distinctions between the moods, which had earlier been more fluid. (Jayasuriya comes to the opposite conclusion, but he seems not to have examined the passages carefully.) The major trend is to replace the optative or the subjunctive with the imperative (or, in the first person, the old subjunctive) in expressing a wish or desire (sometimes specifically characterized as an *āśīs-* 'hope'):

I. 9.1.12 *asyā́m* ṛdhed *dhótrāyāṃ devaṃgamā́yām íti / asyā́ṁ* rādhnotu *hótrāyāṃ devaṃgamā́yām íty evaítād āhāśāste 'yáṃ yájamānaḥ*
'Might he succeed in this oblation going to the gods.' He says by this 'Let him succeed . . .'. Thus does the Yajamāna express his hope.

II. 3.4.24 sám *ahám āyuṣā́ sáṃ várcasā sáṃ prajáyā sáṁ rāyáspóṣeṇa* gmiṣīyéti . . . *ahám etaíḥ* sáṃ gachā *íty evaítād āha*
'Might I be united with long life, with splendour, with offspring, with thriving of wealth.' By this he says 'Let me be united with these'.

These mood substitutions often interact with the tendency to limit modals to the present stem – hence, for example, present imperative/subjunctive *sáṃ gachai* for aorist optative *sám . . . gmiṣīya* in II. 3.4.24; present imperative *vahatu* for aorist subjunctive *vakṣat* in IX. 2.3.20 (quoted p. 170 above); present imperative *rādhnotu* for aorist optative *ṛdhet* in I. 9.1.12.

While the imperative becomes the mood appropriate for wishes, the optative becomes especially characteristic of expressions of obligation and necessity. So, in I. 5.1.26 first-person subjunctive *āvahāni*, now felt as an imperative, is replaced by optative *váheyam* in such a context:

> *víśve devāḥ śāstána mā . . . prá me brūta . . . yáthā vo yéna pathā́ havyám* ā́ *vo* váhānī́ti . . . *ánu mā śāsta yáthā vaḥ . . . havyáṃ* váheyam *íti*
> 'O Viśve Devās, instruct me, proclaim to me how (and) by what path I shall bring you your oblation', (that is) 'instruct me how I should bring you the oblation'.

Complete change in syntactic construction is relatively rare. The clearest examples involve the elimination of a dative infinitive or quasi-infinitive by a recasting of the sentence:

III. 2.2.22 prabúdhe *naḥ púnas* kṛdhī́*ti* yáthā . . . prabúdhyāmahā *eváṃ naḥ* kurv *íty evaítād āha*
'Make us awake again.' He says by this 'Make (it) so for us that we shall awake'.

IX. 5.1.48 *imáṃ yajñáṃ no* naya *svàr devéṣu* gántava *íti . . . svargáṃ lokáṃ* gamayéty *etád*
'Lead this worship of ours to go to the sun, among the gods.' That is, 'make (it) go to the heavenly world'.

Note in III. 2.2.22 also the replacement of the aorist imperative *kṛdhi* with present imperative *kuru*, in conformity with the usual trend.

The most common type of substitution encountered in the ŚB is that of one root for another, but this practice can be further subdivided. There are isolated substitutions, scattered throughout the text. Some of these seem to result from misunderstanding of the glossed form, especially if it is of very low frequency in the Brāhmaṇas, as in I. 4.1.27:

áchā deva vivāsasīti . . . *etán no* gamayéty *evaítád āha*
'Thou dost seek to win (this), o god.' 'Make us go to it' he says by this.

However, such replacements could simply result from a freer paraphrase than we generally meet with. Other isolated substitutions are found where no possibility of misunderstanding exists, e.g. III. 9.3.5:

ūrdhvám imáṃ adhvaráṃ divi devéṣu hótrā yachéti . . . *ūrdhvám imáṃ yajñáṃ divi devéṣu* dhehīty *evaítád āha*
'Hold erect this ceremony, the oblations in heaven among the gods.' He says by this 'place erect this worship in heaven among the gods'.

(Note here the unusual substitution of an aorist imperative for a present.)

In addition to isolated substitutions, there are systematic or widespread ones. The most obvious example is the replacement of *pā́ti* with *gopāyáti* already discussed above. This occurs throughout the ŚB, and though *pā́ti* occurs in unglossed mantras, it does not occur independently in prose, whereas *gopāyáti* is common there.

However, this type of replacement throughout the text is unusual. Instead, it turns out that a number of systematic replacements are confined to a portion of the text, i.e. to the Śāṇḍilya books VI–X. Thus, the rare root *ci* 'perceive' is replaced by *dṛś* 'see' 3 times in Kāṇḍa VI. The *-áya*-formation *pūráya-* replaces forms of *pṛ/prā* 5 times in this section, but elsewhere *pṛṇā́ti, áprāḥ* are unglossed:

(ā́) pṛṇa → *(ā́) pūraya* VI. 5.4.12, VIII. 7.2.6
ā́prāḥ → *ā́pūrayati* VI. 7.3.10, VII. 5.2.27
āpaprivā́n → *ā́pūrayati* IX. 2.3.17

Moreover, in this section certain forms serve almost as cover-term glosses for a range of mantra forms. In *mā́* prohibitives *himṡíḥ* 'do not harm' glosses four different verbs, each with a more specific meaning; 3 of the 4 are in Books VI and VII:

(III. 6.4.13–14 *lekhīḥ* → *himṡīḥ*)
VI. 4.4.4 *abhí śocīḥ* → *himṡīḥ*
VII. 5.1.8 *abhí tāpsīt* → *himṡīt*
VII. 5.2.17 *abhí maṁsthāḥ* → *himṡīḥ*

The height of what one must call absurdity is found with the stem *dī́pya-* and especially participle *dī́pyamāna-* 'shine, shining', which appears as gloss 15 times, sometimes to forms of the root *dī* 'shine' to which it was originally related, but to a wide semantic range of other forms as well, including such divergent ones as *mandasva* 'become exhilarated', *úd iyarṣi* 'thou dost raise up'. All 15 examples occur in Books VI–IX:

VI. 3.3.19 (*rabhasáṃ*) *dṛśānam* → *dī́pyamānam*
VI. 3.3.20 *járbhurāṇaḥ* → *dī́pyamānaḥ*
VI. 4.1.2 *dī́dyatam* → *dī́pyamānam*
VI. 4.2.7 *dīdivā́n* → *dī́pyamānaḥ*
VI. 4.2.9. *śócasva* → *dī́pyasva*
VI. 4.4.21 *śóśucānaḥ* → *dī́pyamānaḥ*
VI. 8.1.14 *dīdā́ya* → *dī́pyamānaḥ*
VII. 1.1.28 *árocathāḥ* → *ádī́pyathāḥ*
VII. 3.1.30 *úd iyarṣi* → *úd dī́pyase*

VII.3.1.31	*mándasva*	→ *dī́pyasva*
VII.3.1.32	*irajyán*	→ *dī́pyamānaḥ*
VIII.6.3.20	*dávidyutat*	→ *dī́pyamānaḥ*
VIII.6.3.21	*dyotatām*	→ *dī́pyatām*
VIII.6.3.21	*vibhrā́jamānaḥ*	→ *dī́pyamānaḥ*
IX.2.3.25	*dī́dyānaḥ*	→ *dī́pyamānaḥ*

These root replacements are not the only feature characteristic of the Śāṇḍilya section. Other peculiarities already discussed are unequally distributed in the text, found much more commonly here. 3 of the 4 examples of *hára-* replacing *bhára-* occur here (III[1], VI[1], VII[1], VIII[1]), 6 of the 7 of *karóti* replacing *kṛṇóti* (I[1], VI[4], VII[1], VIII[1]). In other words, though *kṛṇóti* is no longer a freely produced stem anywhere in the ŚB, it is only in this section that a concerted effort is made to gloss it with its modern counterpart.

Of course, these strikingly different patterns of glossing in the different sections of the text do not lead to equally striking new conclusions about the strata of the text, since the different provenience of the Yājñavalkya and Śāṇḍilya sections has been generally accepted for well over a century. However, this does lengthen the list, begun by Weber (*Ind. Stud.* 13: 1873), of significant linguistic differences between the sections, and also makes the glossing practices within those two different sections seem more self-consistent.

Oddly enough, the type of almost verbatim gloss so common in the ŚB is relatively infrequent in other Brāhmaṇa texts,[4] so that systematic comparison of the development of the Vedic verbal system is not possible by this method. However, I hope in future to examine the Śrauta Sūtra material, which should yield a wealth of data concerning later stages of the verbal system.

Notes

1 All passages cited are from the ŚB (Mādhyandina recension).

2 But note the consistency in voice in the immediately preceding VII. 2.2.4 *sī́rā yuñjanti kaváyo yugā́* ví tanvate *pṛ́thag íti . . . té sī́ram ca yuñjánti yugā́ni ca* ví tanvate *pṛ́thak*.

3 For further discussion of this last type, see my 'Two problems in the inflection of the Vedic intensive', *MSS* 42 (1983) 41–73.

4 Unfortunately I have had access only to a small portion of the Kāṇva recension of the ŚB.

The tenses of the Latin perfect system

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1. One of the most conspicuous features of the Latin verbal system is the contrast between the three tenses of the *infectum*, or present system, and those of the *perfectum*, or perfect system.[1] Thus, a typical primary verb like *tangō, -ere* 'touch' forms a present tense proper (3rd sg. *tangit* 'touches'), a future (*tanget* 'will touch') and a preterite or 'imperfect' (*tangēbat* 'was touching'), which together comprise its present system; systematically opposed to these are the perfect (*tetigit* 'has touched'),[2] future perfect (*tetigerit* (1st sg. *-ō*) 'will have touched') and pluperfect (*tetigerat* 'had touched'), which constitute the corresponding perfectum. This parallelism extends to the subjunctive, where the opposition between imperfect (*tangeret*) and present (*tangat* – better termed 'non-preterite') forms in the infectum is matched by the contrast between the pluperfect (*tetigisset*) and perfect (*tetigerĭt* (1st sg. *-im*)) subjunctives in the perfectum.[3] Similarly, there are two infinitives: *tangere* 'to touch' has its structural counterpart in the perfect infinitive *tetigisse* 'to have touched'.

This state of affairs is quite different from that reconstructible for Proto-Indo-European. Evidence from Indo-Iranian and Greek indicates that the perfect served simply to denote a state in the parent language, without any overt specification of time; although a tendency to create distinct tense forms from the perfect stem is well-developed in Greek and Vedic Sanskrit, the verbal systems of these languages are organized on entirely different lines from that of Latin.[4] The Latin system, however, is not wholly isolated. It has an almost exact counterpart in the Italic dialects, which likewise oppose their present (e.g., Vest. *didet* 'dat', subj. Umbr. *dirsa* 'det') to a perfect (e.g., Osc. deded 'dedit', subj. dadid 'dediderit') and their future (e.g., Osc. *didest* 'dabit', Umbr. *habiest* 'habebit') to a future perfect (e.g., Osc. *fefacust* 'fecerit', Umbr. *habus* 'habuerit'). It is quite possible that the poorly represented Osco-Umbrian imperfect (.e.g., Osc. 3rd pl. fufans 'erant', patensíns 'panderent') was paired with a pluperfect, but no example of this tense is attested.

Although the structural similarity of the Latin and Osco-Umbrian systems presumably reflects a Common Italic feature, the detailed history of the Latin perfect tenses is for the most part obscure. Outside the perfect indicative itself, the perfectum in Latin is characterized by an etymologically opaque element *-er-* (*-is-* before consonants), to which are added the appropriate tense and mood signs – *-i-* for the future perfect, *-ă̆-* for the pluperfect, *-ĭ̆-* for the perfect subjunctive, *-sĕ̆-* for the pluperfect subjunctive and *-se* for the perfect infinitive. No trace of this formative is found in Osco-Umbrian: here the future perfect is characterized by a tense sign *-us-* (Osc. *-uz-*, Umbr. *-ur-* before vowels), while the perfect subjunctive is formed by adding the mood sign *-i-*, *-í-* to the unextended perfect stem.

These forms, themselves problematic, show no obvious connection with the corresponding categories in Latin.

2. Given the absence of clear formal counterparts elsewhere, it is hardly surprising that the majority of attempts to explain the Latin perfect system have been based on internal reconstruction. The results yielded by this method are at least superficially encouraging: each of the tense and mood signs identified above recurs in a similar function in the infectum, where each has a well-known Italic or Indo-European etymology. Thus, the historical thematic vowel of the future perfect appears also in the ordinary futures *erit* 'will be' and *cantābit* 'will sing', and ultimately continues the **-e/o-* of the Indo-European subjunctive; the *-ā̆-* of the pluperfect is identical with the **-ā-* of imperfects like *erat* 'was' and *cantābat* 'was singing' and continues a preterital element known also from Celtic (Benveniste 1951: 19; Jasanoff 1983: 75–82); the *-ī̆-* of the perfect subjunctive is clearly comparable with the vowel of present subjunctives like *sit* 'may be' and *uelit* 'may wish', which rest on inherited athematic optatives in **-i̯ē-/-ī̆*. Even the *-sē̆-* of the pluperfect subjunctive, though ultimately obscure in Indo-European terms, is inseparable within Latin from the *-sē̆-* (after vowels *-rē̆-*) of imperfect subjunctives like *esset* 'might be' and *tangeret* 'might touch'. The formative *-er-* (*-is-*) to which these elements are added is clearly the continuant of an earlier non-alternating **-is-*, which is preserved before the **-sē-* of the pluperfect subjunctive but shows the regular effects of rhotacism and vowel weakening before the vocalic suffixes of the other categories. The historical inference suggested by the Latin facts, therefore, is that forms like *tetigerit, tetigerat, tetigerīt* and *tetigisset* originated as the 'short vowel' subjunctive, *ā*-preterite, athematic optative and *'sē*-subjunctive', respectively, of an enlarged perfect stem in **-is-*. This line of reasoning, essentially non-comparative in character, is accepted by most of the major handbooks of Latin historical grammar (cf. Sommer 1914: 575; Meillet-Vendryes 1953: 265–266; Buck 1937: 297; Leumann-Hofmann 1977: 608ff.).

The source of the enlargement **-is-*, however, remains problematic. According to the usual view, **-is-* is of 'aoristic origin', and came to be generalized throughout the perfectum from a nucleus of verbs in which it served as the distinguishing mark of the perfect (< aorist) stem. Implicit in this analysis is the assumption that alongside the familiar sigmatic aorist in **-s-*, the verbal system of Proto-Into-European also had an aorist in **-is-*; the main evidence for such a category is provided by the *iṣ*-aorist of Vedic Sanskrit. The Vedic formant in question, however, is now known to have originated from the addition of **-s-* to roots or stems ending in a laryngeal: 1st sg. *ápāviṣam* 'I purified' is simply the normal reflex of the *s*-aorist **e-pḗuh$_x$-s-m̥*, while *ástambhiṣam* 'I propped up' is the sigmatized replacement of an earlier aorist **e-stembhh$_x$-m̥*.[5] Thus, the *-i-* of the *iṣ*-aorist is etymologically not a true **-i-* but a vocalized laryngeal, which would have yielded **-ă-* rather than **-i-* in Italic. A direct connection between the Latin and Sanskrit forms is virtually impossible.

Independent evidence for an aorist morpheme **-is-* has also been alleged from Latin itself. It is well-known that the personal endings of the Latin perfect are largely based on those of the perfect of Proto-Indo-European: thus, the first singular ending *-ī̆* (*tetigī̆*, etc.) continues the **-a* (< **-h$_2$e*) of Gk. οἶδα and Ved. *véda* 'I know', augmented by the *hic et nunc* particle **-i*, while the third plural in *-ēre* (*tetigēre* < **-ēr-i*) is related to the Hittite third plural ending *-er* and, more distantly, to the Vedic third plural perfect in *-uḥ* (< **-ṛs*). In the second person, however, the Latin endings are *-istī̆* (sg.) and *-istis* (pl.), which have

commonly been regarded as the historically regular perfect forms (cf. especially Ved. 2nd sg. *-tha*, Gk. *-θa*, Hitt. (*ḫi*-conj.) *-ti* < **-tai*) preceded by an originally autonomous morpheme **-is-*. The validity of this interpretation is questionable. As noted by Cowgill (1965: 172–73) there is considerable comparative evidence for assuming a sigmatic doublet of the second singular perfect ending in the parent language. Lat. *-stī* is difficult to separate from Gk. *-σθα*, Go. *-st* (in *saisost* 'you sowed'), Hitt. 2nd sg. pret. (*ḫi*-conj.) *-šta* and Toch. B 2nd sg. pret. *-sta* (A *-ṣt*). The source of these variants is unclear, but it is natural to speculate that **-st(h)a* (< **-sth₂e*) arose by resegmentation from cases where a root-final dental in contact with the **-t-* of the original ending produced a sibilant by regular sound change (**-TT-* > **-TsT-*).

The conclusion suggests itself, therefore, that the historically correct segmentation of Lat. *-istī* is not *-is-tī*, with an ending *-tī* accompanied by a tense sign *-is-*, but *-i-stī*, with a sigmatic desinence *-stī* preceded by a union vowel *-i-*.[6] Such an analysis is also favored by the overall structure of the perfect paradigm. From a synchronic point of view, none of the perfect endings begins with a consonant – a state of affairs readily intelligible in the first singular, third singular and third plural, where *-ī, -it* and *-ēre* continue **-ai, *-ei*[*t*] and **-ēri*, respectively, but more suprising in the second person and in the first plural, where the regular ending is *-imus*. The latter termination, a replacement of earlier **-mos*, is clearly secondary. Its origin is commonly traced to the reduplicated perfects *dedimus* 'we gave' and *stetimus* 'we stood', which can theoretically be taken from athematic preforms of the type **dedh₃mos* and **steth₂mos* (Sommer 1914: 577–578; Buck 1937: 296; Leumann-Hofmann 1977: 607). This explanation, however, requires us to separate the *-i-* of *-imus* from that of 2nd sg. *-istī* and 2nd pl. *-istis*: the rules of Latin vowel-weakening, which would regularly have allowed the pre-Latin laryngeal reflex **-a-* to develop to *-i-* in open syllables, would have taken a sequence like 2nd sg. **dedastai* to **dedestī*. Phonologically, the *-i-* of *-istī* and *-istis* can only continue an original **-i-*, and it is natural to see this vowel in *-imus* as well.[7] From a typological point of view, *-istī, -imus* and *-istis* resemble nothing to closely as the corresponding Greek perfect endings *-αϛ*, *-αμεν* and *-ατε*, the initial element of which is likewise an accretion of the post-Indo-European period. While the *-i-* of the Latin forms presents far more serious etymological difficulties than its Greek counterpart, its original status as a union vowel is hardly less clear.[8]

This finding, though damaging to the traditional view of the extended perfect system, is eminently consistent with the known tendencies of analogical change. It would be surprising indeed if Latin had systematically excluded an inherited tense sign **-is-* from the first and third person forms of the perfect, while adding it in the second person even to forms which were already sigmatic (cf. *dīxistī* 'you said', etc.); it would be more remarkable still if such an element, though nowhere attested outside Latin, had subsequently become an obligatory constituent of the stem from which all the remaining perfectum forms were created in the post-Italic period. Internal reconstruction, normally an invaluable supplement to the comparative method, would here seem to have led to an impasse.

3. It may be appropriate at this point in our discussion to recall that the elaboration of the perfect system was not a development of Latin alone, but of the Italic branch of Indo-European as a whole. The future perfect and perfect subjunctive of Oscan and Umbrian bear little overt resemblance to the corresponding categories of Latin, but the formal disparity between the two groups can easily be exaggerated. In point of fact, the traditional

tendency to separate the Osco-Umbrian perfectum from that of Latin is valid enough in the case of the perfect subjunctive, where forms like Osc. dadid, *fefacid* 'fecerit' and Umbr. *combifiansi* 'nuntiauerit' are transparently old perfect optatives of a type not found in Latin.[9] The situation in the future perfect, however, is of greater potential interest.

A great deal has been written concerning the origin of the Osco-Umbrian future perfect sign *-us-*, which in the third singular is followed directly by the personal ending *-t* (cf. Osc. *fefacust* 'fecerit', Umbr. *dersicust* 'dixerit') and in the third plural develops regularly to Osc. *-uz-*, Umbr. *-ur-* before the vocalic ending *-ent* (cf. Osc. *angetuzet* 'proposuerint', Umbr. *dersicurent* 'dixerint'). There is little to recommend the old comparison of these forms with the Latin *uī*-perfect, as suggested, e.g., by von Planta (1897: 373ff.) or with the Indo-European perfect active participle in **-u̯os-/-u̯es-/-us-* (so Schultze 1887: 272–4; similarly Buck 1937: 173).[10] Rather, as seen already by Bartholomae (1887: 92), the point of departure for the creation of the Osco-Umbrian future perfect must have been the root *fu-* 'be', where both the appearance of the sequence *-us-* and its eventual reanalysis as an independent morpheme can be easily explained. The future perfect of *fu-* is probably attested in the Umbrian third plural form fefure (for **fefurent*), implying a 3rd sg. **fefust*; it is likely that this was the Common Osco-Umbrian form, and that the absence of reduplication in Osc. *fust* 'fuerit' is secondary. The origin of **fefust* is clear. It is a future perfect of exactly the same type as Gk. τεθνήξει 'will be dead' and λελείψεται 'will be left', in which the productive mark of the simple future (*-σε/o-*) has been added to the perfect stem to produce a new future tense with stative value. In Osco-Umbrian the sign of the ordinary future is a bare *-s-*; **fefust* patently shows the addition of this morpheme to the inherited perfect stem **fefu-* (cf. Ved. 3rd sg. *babhūva* 'was', with secondary *-ū-*).

Once established in **fefust*, the spread of *-us-* to other sigmatic future perfects would have been favored by two important factors. First, the simple perfect corresponding to **fefust* would presumably have had the Common Osco-Umbrian form **fefed* < **fefu̯ed* (cf. Osc. 3rd pl. *fufens*, 3rd sg. **fufed*, with secondary *-u-* from *fufans* 'erant'), so that the **-u-* of **fefust* would early have tended to lose its synchronic status as a constituent of the root. Equally important, the perfect system of **fu-* appeared not only in the free forms **fefust* and **fefed*, but in every Osco-Umbrian verb that had an *f*-perfect – a fact which gains in significance when it is recalled that the *f*-perfect is the only one of the suffixed perfect types to appear in both dialects. The pattern *-ed* (perf.): *-ust* (future perfect) would thus have received support from pairs of forms like Osc. aíkdafed 'decreuit' (?) on the one hand and Umbr. *andirsafust* 'circumtulerit' on the other; the eventual result was the establishment of a proportion **-fed* : **-fust* :: **-ed*: X, which triggered the creation of future perfects like **fefakust* (cf. Osc. *fefacust*), **dedikust* (Umbr. *dersicust*) and *benust* (Umbr. *benust* 'uenerit') from perfects of the type **fefaked* (cf. Osc. *avafakeт* 'dedicauit'), **dediked* and **bened* (cf. Osc. kúmbened 'conuenit'). Such forms apparently supplanted earlier future perfects in simple **-st* (**fefakst*, **dedikst*, etc.), the replacement of **-s-* by **-us-* providing a transparent example of a 'bipartite' morpheme, in Kuryłowicz' sense, taking on the distribution and function of a simple one.[11]

The future perfect of the Italic dialects would thus seem to have originated as an *s*-future built from the perfect stem. In the discussion that follows, we shall examine the consequences of this result for the analysis of the future perfect in Latin, the development of which will be seen to have proceeded along strikingly similar lines.

4. We have already noted that the traditional interpretation of Lat. *tetigerit* as the subjunctive of a quasi-aorist stem **tetag-is-* depends crucially on the untenable assumption of an *is*-aorist in the verbal system of Proto-Indo-European. Let us therefore approach the problem from an Italic, rather than purely Latin, perspective and consider the possibility that in pre-Latin, as in pre-Osco-Umbrian, the future perfect was originally made by adding **-s-* to the perfect stem. A verb like *tangō* would thus have acquired a future perfect stem **tetag-s-*, inflected athematically. The fate of such a stem in Latin would have been closely bound up with that of the *s*-future of the infectum, a formation which survives in early Latin futures like *faxō, -it* 'will do' and *capsō, -it* 'will take'.[12] The *faxō*-type is thematic, presumably because the inherited indicative **faksmi, -ti*, etc. was prehistorically replaced by the corresponding subjunctive. It is natural to assume that the same development would have affected the future perfect, effectively entailing the thematization of **tetag-s-* to **tetag-se/o-*. The regular third person singular of this stem would have appeared in Classical Latin as **tetexit*; similarly, from *agō* 'I lead' and *dīcō* 'I say' the expected future perfects would have been **ēxit* and **dīxit*, respectively. These forms, it will be noted, differ from the attested *tetigerit, ēgerit* and *dīxerit* in only one particular: they lack the etymological **-i-* (> *-e-*) which precedes the **-s-* (> *-r-*) in the historical forms. The problem of explaining the future perfect thus reduces to the problem of explaining the substitution of the stem-type **tetag-i-se/o-* for the shorter and historically predictable **tetag-se/o-*.

An obvious solution emerges from a comparison of our third hypothetical example, **dīxit*, with its attested counterpart *dīxerit*. The verb *dīcō* is one of many in Latin with a perfect that continues an inherited sigmatic aorist (**deik-s-*). The addition of the **-s-* of the future perfect to the already sigmatic perfect stem **deiks-* would regularly, of course, have had no overt phonetic affect: **deiks-s-* would simply have fallen together with **deiks-*, giving the postulated **dīxit* 'will have said'. But *dīxit*, representing older **deik-se-t*, was already current in older Latin as the normal *s*-future of *dīcō* in the present system.[13] The homophony of **dīxit*, or its prototype **deikset* /deiks-se-t/, 'will have said' with *dīxit* < **deikset* /deikse-t/ 'will say' would hardly have been tolerable in a language where the infectum: perfectum opposition was as basic a feature of the verbal system as it was in pre-Latin. A device would clearly have been needed to disambiguate the two forms; it is in this context that the replacement of the future perfect **deikset*, with underlying but unrealized **-ss-*, by **deiksiset*, the source of the attested *dīxerit*, may best be understood. We have seen in section 2 that the union vowel **-i-* was prehistorically introduced into the paradigm of the perfect proper as a means of separating the stem from the consonantal endings. The natural inference is that this vowel was extended from the perfect indicative to fill a comparable function in the future perfect. There are several ways in which the generalization of **-i-* can have proceeded. It is possible, for example, that at an early period, when forms like 2nd sg. **deiks-i-stai* were still in free variation with older forms of the type **deiks-stai* (i.e., [deik-stai]), analogy led to the creation of **deiks-i-se-t* beside **deiks-se-t* ([deikset]). Alternatively, the co-occurrence in early Latin of haplological forms like *dīxtī, mīstī* 'you sent', *intellēxtī* 'you understood', etc. beside regular *dīxistī, mīsistī, intellēxistī* may have led to the back-formation of **deiksiset* from **deikset* in its future perfect function, thus making possible the structurally useful distinction between **deiksiset* > *dīxerit* (future perfect) and **deikset* > *dīxit* (future). In either case, the result would have been the substitution of **-ise/o-* for **-se/o-* as the mark of the future perfect in verbs with *s*-perfects; from here the longer and more convenient allomorph would have been free to spread to forms like

**tetakset* /tetag-se-t/ and **ēkset* /ēg-se-t/, giving **tetagiset* > *tetigerit* and **ēgiset* > *ēgerit*. Viewed in this way, the Latin developments would present obvious parallels to those in Osco-Umbrian, where **-s-* was similarly replaced by a variant suffix of the form **-Vs-*, albeit via an entirely different series of analogical changes.

5. An explanation of the perfect subjunctive follows almost mechanically from the foregoing account of the future perfect. In the infectum, the archaic *s*-future is associated with a present subjunctive, represented by forms of the type *faxim, -īs, -ĭt, ausim* ('I would dare'), *-īs, -ĭt*, etc.; historically, these simply continue the optative of the athematic formation presupposed by the type *faxō*. It is in no way surprising, therefore, that the future of the perfectum is likewise associated with an old optative, the attested reflexes of which show the regular renewal of **-s-* to **-is-* and pattern as perfect subjunctives (cf. *tetigerĭt* < **tetagisĭt*). Sigmatic forms of this type may well have competed for a time with perfect optatives similar to those of Osco-Umbrian (Osc. *fefacid*, etc.), but their success at the expense of the earlier formation was complete by the time of our earliest records.

Unlike the future perfect and perfect subjunctive, which under the above interpretation can be regarded as analogical transformations of categories inherited from Common Italic, the remainder of the perfect system is, at least from a formal point of view, of comparatively recent origin. The historically correct analysis of *tetigerit* and *tetigerĭt* as **tetag-i-se-t* and **tetag-i-s-ī-t*, respectively, is no longer valid for the attested stages of Latin; owing to phonological and other changes, the synchronic form *tetigerit* bears a much closer surface resemblance to *erit* (< **es-e-t*), the third person singular future of the copula, than to its immediate relatives of the type *faxit*. It is probable that this similarity was exploited in the creation of the new pluperfect: given the fact that in the infectum *erit* was opposed to a preterite (imperfect) *erat*, it was a simple matter for *tetigerat* to enter the perfectum as the preterital counterpart of the future perfect *tetigerit*. Comparable developments were in all likelihood responsible for the introduction of the pluperfect subjunctive *tetigisset* and perfect infinitive *tetigisse*; obvious models were provided by the copula forms *esset* (impf. subj.) and *esse* (pres. inf.).[14]

With the establishment in the perfectum of *tetigerat, tetigisset* and *tetigisse*, or their phonological antecedents, the metamorphosis of the sequence **-(i)s-* from a future sign to an ancillary mark of the perfect stem would have been complete. In the resulting system of the historical period, *-er-/-is-* is a purely formal enlargement with no detectable semantic function; its history provides a striking illustration of the vicissitudes to which a grammatical morpheme – particularly a productive one – may be subject in the course of linguistic change.

Notes

[1] I would like to thank Alan Nussbaum for numerous comments on an earlier version of this paper. All errors are naturally my own.

[2] The symmetry of the system, of course, is slightly disturbed by the fact that the perfect may also have the value of a simple preterite; in this sense it presumably reflects the semantics of the Indo-European aorist, of which it is in part the formal continuant as well.

[3] As a formal convention in the discussion that follows, the endings of the 3rd singular perfect subjunctive and 3rd singular future perfect will be cited as *-ĭt* and *-it*, respectively. In practice, the two are confused even in Plautine Latin.

[4] In particular, these languages assign a central role to the present: aorist opposition, which has no counterpart in Latin. It should be noted, moreover, that the Vedic pluperfect is not a true past of the perfect at all, but an ordinary preterite with essentially the same value as the imperfect.

[5] Cf. Narten (1964) s.v. *grabhi-*. The absence of *vṛddhi* is the surest indication that the *-s-* is secondary.

[6] Similarly, the second plural in *-istis* is presumably to be analyzed as *-i-stis*, with *-s-* analogically extended from the second singular.

[7] Note further that the Plautine and Vulgar Latin third plural in *-ĕrunt* (cf. OFr. *distrent*, It. *dissero*, etc.) can straightforwardly be analyzed as **-i-r[onti]*. An active third plural in **-ront* is found also in Tocharian B *-re* (W. Cowgill, personal communication).

[8] The *-a-* of the Greek forms presumably originated in the third plural, where the development of *-ar[ι]* from **-nt* was phonologically regular; compare also the **-u-* which appears in the plural and dual endings of the strong preterite in Germanic (Go. pl. *-um, -uþ, -un* (< **-nt*); du. *-u, -uts*). Latin *-i-*, of course, cannot have arisen in this way; its original locus is not immediately obvious.

[9] A recent account of the Osco-Umbrian perfect subjunctive is given by Lindeman (1982: 303–6).

[10] In the notes to his second edition, however, Buck (1937: 362) abandons this theory for the view taken here.

[11] Cf. Kuryłowicz' first 'law' of analogy: "Un morphème bipartite tend à s'assimiler un morphème isofonctionnel consistant en un des deux elements, c.-à-d. le morphème composé remplace le morphème simple." (Kuryłowicz 1945–49: 125).

[12] The functional status of these forms, which are only occasionally future perfects, is the subject of a forthcoming study by Alan Nussbaum.

[13] Such a form is implicit, e.g., in the Plautine 2nd sg. subj. *dīxīs* (*Capt.* 1.2.46).

[14] It is impossible, of course, to determine whether the attested pluperfect indicative and subjunctive have replaced non-sigmatic predecessors of the type **tetag-ā-t* and **tetag-sē-t*. The group *-ss-* in the pluperfect subjunctive and perfect infinitive makes it likely that Indo-European intervocalic **-s-* was still a sibilant (presumably **-z-*) at the time of the creation of these forms.

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*H_3 in Anatolian

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One of the most serious problems confronting the linguist who is trying to establish the phonological history of Hittite and its sister language Cuneiform Luvian (hereafter simply Luvian) is the syllabary in which these languages are written. The cuneiform syllabary was a borrowed script which was not completely adapted to the needs of writing either language, and it obscures a number of important details. Specifically, there are phonological distinctions which can be reconstructed for Indo-European (and which might be expected in Proto-Anatolian) that the cuneiform syllabary apparently does not make. The problem that concerns us here is whether there was a distinction in Proto-Anatolian or its daughter languages between initial *h_2 and *h_3. Although possible reflexes of both sounds are written with the same signs in the cuneiform syllabary, evidence from Lycian, which is written in an alphabet that can make some of the distinctions which are obscured in the cuneiform syllabary, helps to clear matters up.

It is certainly beyond question that initial *h_2 becomes the sound that is written with signs for H plus vowel in Hittite and Luvian, and that the Lycian sound that corresponds to Hittite and Luvian *h* is a velar, presumably a velar spirant, written with χ (κ), *g* or *q*.[1] Several clear etymologies establish this. One of the best known of these is: Hitt. *hant-* "front" (in *hantī* "separately," *hānza* "front," "in front," *hantezziya-* "first"), Luv. *hantil(i)-* "first," *handawat-* "commander": Lyc. 3rd sing. pret. *χ̃ītewete* "he led" and *χ̃ītawata-* "commander," "basileus"[2] (IE *$h_2$ent-, *h_2n̥t- cf. e.g. Gk. *ἀντί* Lat. *ante*).[3]

The problem is with *h_3. There are several cases in which a Hittite or Luvian word with initial *h* can be compared with words in the other Indo-European languages with an *o that can perhaps be derived from *h_3(e). The best of these equations are: Hittl *hāra(n)-* "eagle" (and Pal. *harā(n)-* : Gk. *ὄρνεον*, *ὄρνις*; Hitt. *hark-* "be destroyed," *harni(n)k-* "destroy," *harga-* "destruction" : O. Ir. *orgaid* "slays"; Hitt. *hāppar* "price," "deal," Hitt. (and Luv.?) *happe/inant-* "rich" : Lat. *opus*; Hitt. *hissa-* "axle, shaft" : Gk. *οἴαξ*. But there is a certain amount of evidence suggesting that *h_2 did not color a neighboring *o and *h_3 can only be reconstructed for any of these words when the o-vocalism of the non-Anatolian languages is completely anomalous and there is virtually no possibility that it is analogical. It is extremely difficult, however, entirely to exclude an o-grade, whether original or analogical, in many cases. The word for "sheep" illustrates the problem. On the basis of Luvian *hāwīš* (H. Luv. *hawa/i-*) and Lycian *χava-* we can reconstruct an initial laryngeal for the Indo-European word. The o-vocalism of Greek *ὄϊς* (Hmc. *οἶς*), Latin *ovis*, and O. Ir. *oi* suggests that the laryngeal can be *h_3 (i.e. *$h_3$ewi-), but *$h_2$owi- is also possible.[5]

Even if any of these words can be confidently reconstructed with *h_3, it is not clear whether *h_2 and *h_3 remained distinct phonemes in Anatolian or whether they merged in a single h-like sound. Unfortunately, the writing system offers only ambiguities on this point, since the reflex of *h_2 and possible reflexes of *h_3 are both written with the same series of signs.

It has also been claimed that *h_3 was lost in Anatolian, and there are several plausible looking etymologies which seem to support this. For example: Hitt. *aniya-* "work," Luv. *ānniya-* id. (Pal. *aniya-*): Lat. *onus* (*$h_3$en-); Hitt. *ārra-* "podex": Gk. ὄρρος (*$h_3$erso-); *ār-* "arrive," *ar-* "place onself," *arāi-* "arise" : Gk. ὤρορε, ὦρτο, ὄρνυμι, Lat. *orior* (*$h_3$er-); *arki-* "testicle" : Gk. ὄρχις (*$h_3$ergh-), and perhaps *ariya-* "consult an oracle" if this belongs only with Latin *ōrō, ōrāculum* and not also with Greek ἄρα, ἀράομαι.[6]

One family of words, however, Hittite *hāppar* "business transaction," "price" and its cognates in Anatolian and the other Indo-European languages, provides evidence that initial *h_3 was not only retained in Anatolian, it was in fact distinct from *h_2.

The o-vocalism of the Italic s-stem *opos in Latin *opus* and the denominatives *operor*, Osc. *úpsannam* "operandam," "faciendam" and Umbr. *osatu* "operator," "facito" points to initial *h_3 while Sanskrit *apas-* and Avestan *apah-* indicate that the s-stem *$h_3$epos, *$h_3$epes- was Indo-European.[7] Italic *opos is found beside a related root noun *ops (attested in the abl. sing. *ope*, and the nom. pl. *opes*, and implied by the nom. sing. *Opis*).[8] *Ops is found also in the compounds *cŏps (acc. sing. *cŏpem*, abl. sing. *cŏpī*),[9] *cŏpia*, and *inops*, and in Old Irish in the adjectives *somme* "rich" and *domme* "poor" (*su-/dus-op-smiyo-). Taken by itself, *ops can continue *$h_3$eps, *$h_3$ops, or *$h_2$ops. If the latter were the correct reconstruction, then it might be possible that the o-vocalism of Italic *opos was analogical from *ops.[10] The meaning of the two words, however, was not very close in Italic, and they seem to reflect a semantic split that must have taken place within Indo-European.

The semantic split is reflected in the other Indo-European languages. On the one hand, there are words from *$h_3$ep- which refer to "work" or "action" such as Italic *opos, Indo-Iranian *apas-, Old Norse *efni*, "material," *efna* "work," Old English *efnan* id. (Gmc. *aƀnia-, denom. *aƀnjan), Old High German *uoben* "do, practice, celebrate," Old Saxon *obian* "celebrate" (Gmc. *ōƀjan, cf. Mod. Germ. *üben*), and Old High German *uobo, lantuobo* "farmer," *uobāri* "farmer." On the other hand, many of the Indo-European languages attest words from *$h_3$ep- which mean "wealth or "abundance," perhaps as the products of work or action. In addition to Italic and Celtic *ops, these include, for example: Sanskrit *apnas-* "wealth," Avestan *afnavant-* "having wealth," and perhaps Lithuanian *ãpstas* "abundance," *àpstus* "wealthy."[11]

The meaning "wealth," "abundance" is also found in Anatolian, in Hittite (and Luvian?) *happenant-* or *happinant-* "rich," and in Hittite *happe/inēss-* "become rich," and *happe/ināhh-* "make rich." A primary verb from *$h_3$ep- is perhaps found in the hapax *ha-ap-zi* "be abundant (?)" (KBo XI 34 L. 5). *Hāppar* "price," "deal" (e.g. OH *ha-a-ap-par* KBo VI 2 II 51, *ha-a-ap-pa-ra-az* ib. II 54, *ha-ap-pa-ri-* KUB XXIX 29 Vs 11[12] and the denominatives *happariya-* "hand over" (OH *ha-ap-pa-ri-e-nu-un* KBo III 22 L.20), and *happarāi-*, *happirāi-* "sell"[13] provide an interesting middle ground between the ideas of "wealth" or "abundance" and action which results in wealth. The commercial meanings of these words presumably reflect the fact that commerce was more sophisticated in second millenium Anatolia than it was in the rest of the Indo-European speaking world.[14]

A noun spelled *ha-ap-pi-ir* is found in a Neo-Hittite text. KUB IV 3 + KBo XII 70 (*ha-ap-pi-ir* Rs 14, *ḫa-ap-pi-ir-ma* Rs 15). *Happir* clearly means "price" (nu-us-si ha-ap-pi-ir pe-[es-ke] "give him a price" Rs 14), and seems, therefore, to be related to *ḫāppar*. Another Anatolian noun from *h$_3$ep- is Hittite *ḫāppiriya-* (NH also *ḫāppiri-*) "city," "settled place." This is usually written with the ideogram *URU* (e.g. *URU-ri* KBo VI 2 I 7, Kbo XX 64 Rs 11, *URU-pi-ra-as* KUB XXXVI 62 L. 8, *URU-az* KBo III 22 L. 5, *URU-ya-an* ib. L. 55, *URU-ri-ya-*[*an*] ib. L. 70),[15] but a dative is spelled out as *ha-a-ap-pi-ri* in KBo V 6 I 17 (Neo-Hittite).[16] The meaning "city" or "settled place" perhaps indicates that the *ḫāppiriya-* was originally a market place,[17] or the meaning may be derived from the idea of a "worked over," "tilled" or "built up" place (cf. e.g. OHG *uobo* "farmer, colonus").

That *ḫāppar* and its cognates did have initial *h$_3$ and that *h$_3$ remained distinct from *h$_2$ is confirmed by the Lycian cognate *epirije-* "sell." The word is found in the third person singular active, *epirijeti* (TL 111), and it was originally identified by Laroche (1958, 171–172). Since it is clear that initial *h$_2$ resulted in a Lycian spirant, the initial vowel of *epirije-* suggests that unlike *h$_2$, *h$_3$ was lost in initial position in Lycian. At the very least, this means that initial *h$_2$ and *h$_3$ were distinct phonemes in Anatolian.

It can, however, be argued that the loss of the initial laryngeal in *epirije-* is due to a secondary sound change, perhaps one peculiar to Lycian. Tischler (1980, 510 n. 52), who notes the importance of *epirije-*, prefers this explanation, and Oettinger (1979, 353 n. 200) suggests that *epirije-* has a prothetic vowel. There is, however, no evidence to suggest that prothetic vowels developed before *p in Lycian (cf. *pri-, prije-* and *pere* from *pr(e)i- and *pro[18]). More importantly, several considerations indicate that the root vowel of the noun from which *epirije-* was derived had the full-grade.

The stem of *epirije-*, of course, recalls the stems of *happirāi-* "do business," *ḫāppiriya-* "city," "settled place" and the hapax *happir* "price." For the vowel of the initial syllable, the correspondence Hittite *a* : Lycian *e* is well known. In other examples the Hittite and Lycian vowels are from a full-grade *e, *o or *a (cf. e.g. Hitt. *kweras, *kweran* : Lyc. *tere, terñ* < *k^{W}er-; Hitt. *pāt- (patā-)*, Luv. *pāt-* : Lyc. *ped-* < *pōd- or *pēd-; and Hitt. *āppan*, Luv. *āppan*, Lyc. *epñ* < *apm).[19]

The resemblance between *epirije-* and *happirāi-*, however, is probably more or less coincidental since *happirāi-* is clearly more recent than *happarāi-*. It is first found in Middle Hittite texts (e.g. *ha-ap-pi-ra-*[*si*] KUB XXIII 77 + L. 64; *ha-ap-pi-ra-at-te-ni* KUB XXIII 72 Rs 58) while *happarāi-* is attested in the Old Hittite copy of the first part of the Law Code (3 sg. pres. *ha-ap-pa-ra-iz-zi* KBo XIX 1 II 17; 3 sg. pret. *ha-ap-pa-ra-a-it* KBo VI 2 II 52).[20] This suggests that *happirāi-* is a replacement for Old Hittite *happarāi-*. The most likely source for the vowel of the second syllable of *happirāi-* is contamination with *ḫāppiriya-* "city," "settled place."

Although Oettinger (1980, 147–149) reconstructs a nominative *h$_2$p-ēr and claims that this is directly attested in *happir*, the hapax *happir* probably also does not have anything directly to do with *epirije-*. The text in which *happir* is found, KUB IV 3 + KBo 70, is not particularly old, and there is little reason to believe that it preserves archaisms. In the Inhaltsübersicht for KBo XII 70, Otten suggests that the text, which is a copy of Akkadian omens, is a school exercise. It seems very unlikely, therefore, that *happir* represents an archaic form of the word for "price," or that it reveals very much about the Anatolian or Indo-European paradigm of *ḫāppar*. Instead, *happir* can probably be considered a mistake,

perhaps based on a folk or spelling etymology to *hāppiriya-* "city, settled place" and *happirāi-* "do business."[20]

Hāppiriya- itself appears to continue something original, and it (along with *happiriya-* "hand over") is probably the word that is best compared with *epirije-*. In phonological terms, the vowels of the second syllables of *epirije-* and *hāppiriya-* can continue Indo-European *i since a correspondence Hittite *i* : Lycian *i* is found in Hittite **mēlit* (Post OH *milit*)[21] : Lycian *melite* (μελιτη) < Indo-European *melit. A Proto-Anatolian *hap-ir (Indo-European *h3ep-ir), however, would be very unlikely in morphological terms. The vowel of the second syllable cannot continue Indo-European *e if a correspondence between Hittite *e* and Lycian *e* (< *a) is found in Hittite *kweras, *kweran* "field" (*kwer-* "cut") : Lycian *tere, terñ* "division of an army." Unaccented *e would seem to be ruled out by the correspondence Old Hittite *i* : Luvian *a* in Old Hittite *nēpis* : Luvian *tapassa-* (Indo-European *nebhes-), since this implies that unaccented *e became *a (> Lyc. *e*) in the Luvian branch of Anatolian. The vowel of the second syllable can, however, continue Indo-European **ē*. This is indicated by the correspondence Old Hittite *i* : Luvian *i* or *ī* : Lycian *i* seen in Old Hittite *kissar* : Luvian *iśsari-, isri-* : Lycian *izri-*. The Anatolian words come from an earlier *ghēs-r-, since *ghes-r- should result in Luvian *iyass(a)r- with *a* rather than *i*. For the reflex of *ghe- in Luvian, cf. *tiyammi-* from the locative stem *dhgh-em- beside nominative-accusative *dhegh-ōm in Hittite *tēkan*.

We can probably reconstruct Proto-Anatolian *h3ap-ēr as the noun from which *epirije-* and *hāppiriya* are derived, since the plene writing in NH *ha-a-ap-pi-ri* points to a full-grade accented root. Although a double full-grade, *h3ap-ēr (Indo-European *h3ep-ēr) would be unlikely morphologically, the full-grade root *h3ap- can be analogical from the related *h3ap-ar (Hitt. *hāppar*, Indo-European *h3e/op-r̥), which is surely not an Anatolian innovation. Anatolian *h3ap-ēr then, would represent an earlier *h3p-ēr. In functional terms, *h3pēr could well be a collective beside singular *h3e/op-r̥.[22]

The relationship of the stem *happe/in(a)-* to *hāppar* and *h3apēr is not entirely clear.[23] In the first place, the vowel of the second syllable of *happe/in(a)-* is written with the ambiguous sign *PI* (i.e. *ha-ap-PI-na-*), and it cannot, therefore, be determined whether *happe/inant-, happe/inèss-* and *happe/ināhh-* had a stem with *e* or *i*. An Old Hittite *happēn-* should continue an earlier *h3(e)p-én- (cf. *witēn-* e.g. in NH dat.-loc. sing. *ú-i-te-e-ni* KBo V 2 II 12 from *wed-én- for *ud-én- beside nom.-acc. *wātar* < *wod-r̥). If *ha-ap-pe/i-na-* can be read as *happēn(a)-*, then it could continue the oblique or locative stem of *h3e/op-r̥ or *h3p-ēr, beside *h3ep-n- (for *h3p-n-) in Indo-Iranian *apnas- and Germanic *aƀnia-. However, if the vowel of the second syllable was Old Hittite *i* (i.e. *hāppin-*) then the word could not be derived from an r/n-stem.

Unfortunately, it does not seem to be possible to determine whether *happe/inant-* is Luvian as well as Hittite. The word is found once with the "Glossenkeil" (KUB XVII 24 II 17, NH), but this does not by any means guarantee that *happe/in(a)-* is a genuine Luvian word.[24] An accusative plural with the Luvian ending *-anza, ha-ap-pe/i-na-at-ta-an-za* is found in KUB XXXVI 49 IV 9, a text which is not otherwise especially Luvian looking.[25] This does not, however, provide any more certain evidence that the word is really Luvian, since the Luvian accusative plural endings are sometimes found with stems that are clearly Hittite (cf. e.g. *i-da-a-la-u-wa-an-za* KUB XXIX 7 II 29 with Hittite *idālu-* instead of Luvian *ādduwāl-*.

This is not simply hairsplitting. It is otherwise well known that *e should result in Luvian *a*, and this indicates that if *happe/inant-* is a genuine Luvian word, then it cannot continue *h_3(e)p-en- from an r/n-stem. However, if *happe/inant-* is Luvian, then the vowel of its second syllable can be from *ē, *ǐ or *ei. Since *h_3(e)p-ēn- would not be likely in morphological terms, then *happe/inant-* should be from *h_3(e)p-ǐ-no- or *h_3(e)p-ei-no. In this case, Luvian *happinant-*, and most likely Hittite *happinant-*, would be from a derived adjective in *-ǐ-no- or *-ei-no- "pertaining to wealth," "wealthy" of the type seen in Sanskrit *devǐnah* and Latin *dīvǐnus*.

To sum up: It is well known that initial *h_2 was preserved in Hittite and in Luvian-Lycian. Lycian *epirije-* "sell" and Hittite *hāppiriya-* "city," "settled place," which can be derived from an Anatolian *$h_3$ap-ēr (for *h_3p-ēr) furnish evidence for the history of *h_3. The correspondence Hittite *h* : Lycian zero in these words shows that initial *h_2 and *h_3 remained distinct in Proto-Anatolian and in the Luvian-Lycian branch of Anatolian. Although direct evidence for a distinction between *h_2 and *h_3 in Hittite is not available because of the limitations of the writing system and it is possible that they had merged in a single sound written with the signs for *H* plus vowel, this is not very likely since Hittite is usually more conservative than Luvian-Lycian.

However, since *h_3 was preserved in Hittite, whether or not it was distinct from *h_2, etymologies which depend on a loss of initial *h_3 have to be revised. In many cases, this is relatively simple. For example, *ārra-* and Greek ὄρρος can be derived from *$h_1$orso-, *ār-, ar-, arāi-* while Greek ὤρορε, ὦρτο and ὄρνυμι and Latin *orior* can be from *$h_1$er-, and *arki-* and Greek ὄρχις can continue *$h_1$orĝh-.[26] *Ariya-* "consult an oracle" can be from *h_1i̯-ye/o- and related to Homeric ἐρέω, εἴρομαι "ask" (*h_1r-eu-, *h_1r-u-) rather than to Latin *ōrō, ōrāculum*.

Finally, the equation Hittite *h* : Lycian zero has further implications for our understanding of Indo-European phonology. The initial consonant of Lycian *χava* and Luvian *hāwǐs* can now be seen clearly to continue *h_2 instead of *h_3. Therefore, the o-vocalism of Greek ὄϊς, Latin *ovis* and Old Irish *oi* points to an o-grade root, and this in turn provides an additional piece of solid evidence that *h_2 did not color a following *o.[28]

Notes

1 For the writing of Lycian reflexes of laryngeals and for the transciption of Lycian *k* as χ, see Houwink Ten Cate (1961, 111–112) and Ševoroškin (1968, 168).

2 See Heubeck (1979, 247–260) on the meaning of *χñtawata*.

3 The correspondence Hitt. *h, hh* : Lyc. χ, *g* or *q* is, of course, also found in medial position, e.g. Hitt. *tarh-, tarhu-, taruh-* "conquer, prevail over," Luv. ^{D}*Tarhunt-* "the Storm God" : Lyc. *trqqas, trqqis, trqqñt* id. (IE *trḥ$_2$(u)-, or Luv. 1 sg. pret. ending *-ha* (Hitt. *-hhun*) : Lyc. *-xą, -gą* (Anatol. *-h_2a). The details of this, however, are much less clear than the development in initial position. It is also not clear whether the different Lycian spellings reflect a split, but see, for example Ševoroškin (1968, 168) and the very important findings of Davies (1982/3) on lenition in Luvian.

4 Evidence for a lack of coloring of the vowel in the sequences *h_2o and *oh_2 is summarized by Beekes (1972). Alternative views are those of Ruijgh (1970/71), Kortlandt (1980, 127–129) and Lindeman (1981, 23–31).

5 For example, Oettinger (1979, 367 n. 212) reconstructs *$h_2$owi-. For *$h_2$owi- as a possibility, see Beekes (1972, 129 with n. 14) and Rix (1976, 69). *$h_3$ewi- is reconstructed by, for example, Lindeman (1970, 35 and 1982, 27), Puhvel (1965, 88 and 92), Hamp (1978, 67 n. 7), Rix (1976, 46 and and 146), Schindler (1969, 153 n. 60) and Tischler (1980, 502 n. 36).

[6] For *h_3 in *aniya-* see, for example, Oettinger (1979, 345) and for *h_3 in *ār-* and *ar-*, see Rix (1965, 28–29 n. 12 and 1969, 92–93), Eichner (1979, 84) and Oettinger (1979, 369, 404 n. 13, 479, 523 and 546). Initial *h_3 is reconstructed for *UZU ārra-* and for *arki-* by Tischler (1980, 502). Eichner (1973, 81–82 and 1979, 87) and Oettinger (1979, 43) reconstruct *āk(k)-* "die" with *h_3 (pf. *h_3e-h_3-ok- : *h_3e-h_3k-) and compare Gk. ὠκύς, Skt. *āsu* and Lat. *ōcior*, but this etymology is not very convincing semantically. *Ariya-* was connected with Lat. *ōrō* by Pedersen (1934, 47–48) and Tischler (1977, 56–57 with Pokorny, 781) adds Russ. *oriti*, Skt. *āryate* and Gk. ἄρα and ἀράομαι. However, if the *ōr-* of *ōrō* and *oriti* does continue *$h_3$er-, then ἄρα and ἀράομαι do not belong here, since the a of these words should be from *h_2(e)r-.

[7] *Apas-* is found beside a noun *āpas-* "religious work" (2 Vedic examples: see Grassmann (1955, 174)). The *ā* of *āpas-* recalls the *ō of Osc. 3 sg. perf. *upsed* "fecit," 3 pl. *uupsens* "fēcērunt" and OHG *uoben*, OS. *obian* etc.

[8] Ernout-Meillet (1959, 463–464).

[9] Ernout-Meillet (1959, 464).

[10] This is suggested by, for example, Ernout-Meillet (1959, 466).

[11] Szemerényi (1964, 146 with n. 3) prefers to derive the Lithuanian words from *ap(iē)* "around" plus *steh$_2$- "stand." Greek ἄφενος, a word which has been compared with *happe/inant-* and *apnas-* etc., is most likely a loanword from an Anatolian language. See Szemerényi (1964, 147) and Tischler (1977, 165) with references.

[12] For the analysis of *ha-ap-pa-ri-us* in KUB XXIX 29 Vs 11 as dat. sg. *hāppari* plus the acc. sg. enclitic pronoun *-us*, see Neu (1974, 107).

[13] For the meanings of *happariya-* and *happarài-/happirài-*, see Oettinger (1979, 352–353).

[14] See Neu (1980) on various terms associated with commerce in Hittite Anatolia.

[15] *Happiran* in KUB XIII 2 IV 16, which Neu (1974, 108 n. 244) lists as a form of *hāppiriya-* "city," should probably be a participle from *happiriya-* "give over" = "sell" (i.e. *happir[iy]an*). The passage in which it occurs reads: IV 15 na-as-ma-za da-a-an ku-is-ki ku-it-ki har-zi (16) na-as-ma-za ha-ap-pi-ra-an ku-is-ki ku-it-ki har-zi "oder jemand etwas (weg)genommen hat (16) oder jemand etwas verkauft hat" (translation, von Schuler, 1957 p. 51).

[16] For the analysis of *hāppiriya-* as an *-iya-* (*-iyo-) stem from which an i-stem *hàppiri-* was back-formed, see Neu (1974, 108–109).

[17] See Neu (1974, 108) for this meaning.

[18] For Lyc. *pere*, and Hitt. *parā* from *pr̥ŏ, see Kimball (1983, 766–777).

[19] For Lyc. *tere* : Hitt. *kweras*, see Pedersen (1945, 47–50). According to Laroche (1967, 57–59) *tere tere* is a different word, "everywhere." Hittite *pāt-* (patā-) should, of course, continue *pŏd-, and Luvian *pāt-* and Lycian *ped-* can be from *pōd- or *pēd-. For the long vowel, see Kimball (1983, 864). For Hittite and Luvian *āppan* and Lycian *epñ* from *apm̥, see Kimball (1983, 138 and 766).

[20] It is very unlikely that the spelling *ha-ap-pi-ir* (or *ha-ap-pe-er*) indicated that the sign *PAR* can sometimes be read as *PI-IR (PE-ER)* as is claimed by Oettinger (1980, 149). Although a few examples of an apparent alternation *PAR* ~*PI-IR* or *PE-ER* are found, they are by no means conclusive, and they may admit of other explanations. See Kimball (1983, 131).

[21] For Post Old Hittite *mi-li-it* for Old Hittite **mēlit*, see Kimball (1983, 13).

[22] Another example of a collective in *-ēr might be Hittite *hasduēr* "twigs, brush"; see Schindler (1970, 9).

[23] *Happe/in(a)-* has been considered either a form derived from the oblique stem of an r/n-stem of which the nom.-acc. is represented by *hāppar*, or a form in *-e-no-. See the references provided by Tischler (1977, 165–166).

[24] See Güterbock (1956, 131) on the unreliability of the Glossenkeil here.

[25] Hoffner-Güterbock (1980, 21) consider KUB XXXVI 49 an Old Hittite text with the old ductus, but linguistically, the text is not especially archaic looking.

[26] For *$h_1$orso- see, for example, Eichner (1974, 54). Initial *h_1 in *ār-* etc. and in Skt. *āra* and Gk. is indicated by the ἐ of ἔρετο·ὠρμήθη (Hesych.) and the second element of Λα-έρ-της. For *$h_1$orĝh- in *arki-* and ὄρχις see Watkins (1975, 12) and Beekes (1969) -- with reservations.

[27] See Kimball (1983, 632 n. 111) for this etymology.

[28] Another example of *h_2o resulting in *o* rather than *a* would be Gk. ὀστέον, Lat. *os* : Hitt. *hastāi-*, Luv. *hāssa-* etc. The Greek and Latin words should continue an o-grade *$h_2$ost- since the *a* of Welsh *asgwrn* (*$h_2$est-) points clearly to *h_2 (Lionel Joseph, personal communication).

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The two senses of the term 'anaphora' and their functional unity: evidence from the Rigveda

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0. The term 'anaphora', when applied to linguistic contexts, has from classical antiquity been used in two distinct senses. The earliest securely datable employment of the term in linguistic value given by Liddell-Scott-Jones in their Greek-English Lexicon is the adjectival form *ἀναφορικός* appearing in the *Τέχνη* of Dionysius Thrax (second century B.C.). In listing various types of nouns (including proper noun, epithet, homonym, synonym, eponym, ethnic, and pronoun[1]) he mentions the "anaphoric [sc. noun] . . . , which is also called homoiomatic [i.e. denoting resemblance] and deictic and antapodotic [i.e. correlative]" (*Ἀναφορικόν* [sc. *ὄνομα*] . . . , *ὃ καὶ ὁμοιωματικὸν καὶ δεικτικὸν καὶ ἀνταποδοτικὸν καλεῖται* Uhlig 1883: 40). He next characterizes the anaphoric noun as signifying likeness (*τὸ ὁμοίωσιν σημαῖνον*), giving as examples the Greek pronouns *τοιοῦτος* 'such (a)', *τοσοῦτος* 'so great, so many, so much, etc.', and *τηλικοῦτος* 'so great, so old'. This same usage is found three centuries later in the second century A.D. treatise *Περὶ Ἀντωνυμίας* of Apollonius Dyscolus. The latter, however, distinguishes between deictic and anaphoric roles, noting that "every pronoun is either deictic or anaphoric" (*Πᾶσα ἀντωνυμία ἢ δεικτική ἐστιν ἢ ἀναφορική* Schneider 1878: 9), but that "first and second person pronouns are only deictic, third person pronouns both deictic and anaphoric" (*αἱ κατὰ πρῶτον καὶ δεύτερον μόνως δεικτικαί, αἱ κατὰ τὸ τρίτον καὶ δεικτικαὶ καὶ ἀναφορικαί* Schneider 1878: 9–10). For both of these important ancient grammarians, therefore, 'anaphora' meant resemblance of reference or referential identity treated as a function of pronouns.

Beside the usage of 'anaphora' just described within the field of grammar, the term was employed in a different sense within the separate tradition of classical rhetoric. Here it meant not referential identity, but rather repetition of a word, especially at the beginning of successive clauses. The earliest reference to 'anaphora' (this time the noun) in this sense is found in the treatise *Περὶ Ἑρμηνείας* 'On Style' of one Demetrius, dating from the first century, either B.C. or A.D.[2] Demetrius states that Sappho "sometimes makes graceful use . . . of anaphora" (*Χαριεντίζεται δέ ποτε ... ἐξ ἀναφορᾶς* Roberts 1902: 138), citing the passage *Ἕσπερε, πάντα φέρεις, φέρεις ὄϊν, φέρεις αἶγα, φέρεις ματέρι παῖδα* 'O Hesperus, thou bringest all things (to rest). Thou bringest the sheep, thou bringest the goat (to the fold), thou bringest the child to the mother'. He then notes that "the charm of this passage lies in the [repetitious] statement of the [word] '*φέρεις*', which repeats [i.e. refers repeatedly to] the same thing" (*ἡ χάρις ἐστὶν ἐκ τῆς λέξεως τῆς 'φέρεις' ἐπὶ τὸ αὐτὸ ἀναφερομένης* Roberts 1902: 138). At a later point in the same essay he ascribes a powerful effect to "what is called anaphora" (*ἐκ τῆς ἀναφορᾶς καλουμένης*), citing the lines of

Aeschines, *ἐπὶ σαυτὸν καλεῖς, ἐπὶ τοὺς νόμους καλεῖς, ἐπὶ τὴν δημοκρατίαν καλεῖς* (1902: 190) 'Against yourself you summon him, against the laws you summon him, against the democracy you summon him'. Finally, he notes that this aforementioned figure is threefold: "It is epanaphora . . . on account of the repetition of the same word at the beginning of each clause, and asyndeton, because it is uttered without conjunctions, and homoioteleuton, on account of the recurrent termination *καλεῖς*" (1902: 190).

We are left, then, with two clearly related but distinct senses of the term 'anaphora', and we can speak of anaphora qua repetition and anaphora qua coreference, with the latter to be understood as repetition of reference with or without lexical repetition. Although both of these senses are widely used at the present time, the only sense of 'anaphora' in use in English until the twentieth century was the rhetorical one (anaphora qua repetition). For instance, the Oxford English Dictionary lists as its only definition of the term "the repetition of the same word or phrase in several successive clauses" (Murray, et al 1933: 307). The first reference to anaphora qua coreference is found in the 1972 supplement to the Oxford English Dictionary, where it is defined as "the use of a word which refers to, or is a substitute for, a preceding word or group of words" (Burchfield 1972: 82). The citation given is from Bloomfield (1933: 266): "The word *one* . . . replaces *a* with anaphora of the noun when no other modifier is present (Here are some apples; take one)". It is not hard to determine that Bloomfield took the term in this sense from Jespersen (1914: 247), who is cited in Burchfield (1972) as having been the first to employ the adjective 'anaphoric', applying it also to the term 'one' "if it refers to some word already mentioned" (Burchfield 1972: 82). Jespersen may have in turn gotten the term in this sense from Windisch (1869), who employed it with reference to the citation from Apollonius Dyscolus provided above.[3] Over the past twenty years or so 'anaphora' in the sense 'coreference with what precedes' has become widely used by linguists. At the same time, however, it continues to be employed by classical philologists in the sense 'lexical repetition'. Specialists in each of these areas may not always be aware of the different senses in which 'anaphora' is used. It therefore seems fitting, in a volume honoring Professor Henry M. Hoenigswald, whose scholarship encompasses both classical philology and linguistics, to point out this difference. However, in the remainder of this paper we shall demonstrate, using data from the Rigveda, that the two senses of 'anaphora', although different on a purely descriptive level, are equivalent on the level of discourse structure.

1. That anaphora qua coreference and anaphora qua repetition are closely related in function is hardly a new idea. As noted by Halliday-McIntosh-Strevens (1964: 248), grammatical anaphora, grammatical substitution, and lexical anaphora are all features of textual cohesion. Of these, the first is anaphora qua coreference, the third anaphora qua repetition, and the second is described as "the use of 'do' and 'one' . . . , as in 'I might do' and 'a big one'". Again, in discussing Indo-European discourse structure Bader (1979: 129) states that "l'anaphore a deux définitions (lien avec ce qui précède; répétition)". These two subtypes of anaphora intersect in those instances where the repeated lexical element is itself a pronoun coreferential with its predecessor, e.g. RV I.100.13c *táṃ sacante sanáyas táṃ dhánāni* 'That one do the attainments accompany, that one the prizes'. The role of anaphora within this passage is inherently conjunctive; it at once demarcates the onset of the individual clauses and binds them together, through both repetition and coreference, in the absence of overt conjunction (cf. Demetrius' statement linking the figures anaphora, asyndeton, and ho-

moioteleuton). In most instances in the Rigveda coreferential sequences do not involve lexical repetition. This is especially true of the correlative structure *yá- . . . sá/tá-*, which represents a fundamental discourse strategy, as in I. 164.23d *yá ít tád vidús té amṛtatvám ānaśuḥ* 'The ones who know that, those have attained immortality'. It will be evident, however, that considered from the point of view of discourse function, the *yá- . . . sá/tá-* structure of this latter passage is precisely equivalent to the *tám . . . tám* structure of I. 100. 13c. In each case the coreferential terms demarcate and hence bridge the asyndetic gap between two adjacent clauses. Indeed, iteratively anaphoric terms need not be inherently referential. Preverbs, negative particles, other adverbials and verbs may be so used as well with identical effect, e.g. VII. 86.5ab *áva drugdhāni pítryā sṛjā no / (á)va yā́ vayám cakṛmā́ tanū́bhiḥ* 'Release from us the sins of our fathers (and) those which we have committed by ourselves', where the repetition of the preverb *áva* works in concert with the metrical structure both to demarcate and bind together the members of the compound object of *áva sṛj* 'release'.

The contention that anaphora qua coreference and anaphora qua repetition are really one and the same on the level of discourse structure would be substantially confirmed if we could find in natural language some linguistic form that signaled or was associated with anaphora in both of these senses. In fact, the Rigvedic particle *u* is such a form. As I have shown (Klein 1978a, 1985 part 2: 6–62), this particle finds its fundamental employment within coreferential sequences of the type *yá- . . . sá/tá-*, noun . . . *sá/tá-*, and *sá/tá- . . . sá/tá-*, occurring in second or anaphoric position within these sequences and, through an inversion, in initial or annunciatory (more specifically, cataphoric[4]) position. Examples are the following:

VIII. 21.9ab *yó naḥ . . . / prá vásya āninā́ya tám u va stuṣe* 'The one who has led us forth unto what is better, that one do I praise for you.'

VIII. 96.6ab *tám u ṣṭavāma yá imā́ jajā́na / víśvā jātā́ny ávarāṇy asmāt* 'That one shall we praise, who has begotten all these races lower than he.'

VIII. 44.6c *agním īḷe sá u śravat* 'Agni do I summon. (And) he shall listen.'

II. 35.3cd *tám ū śúciṃ śúcayo dīdivā́ṅsam / apā́ṃ nápātam pári tasthur ā́paḥ* 'That one, the pure, shining Apām Napāt, have the pure waters surrounded.'

IV. 8.4ab *sá hótā séd u dūtyàṃ / cikitvā́ṅ antár īyate* 'That one is the priest. (And) that one, understanding, goes between (heaven and earth) on his embassy.'

II. 20.4a *tám u stuṣa índraṃ tám gṛṇīṣe* 'That one, Indra, do I praise, (and) that one do I sing.'

As we have noted above, the sequence *tám . . . tám* is anaphoric in both senses, and when accompanied by *u* following the second occurrence of the pronoun, becomes the channel whereby the particle expands its range of occurrence from referentially anaphoric to iteratively anaphoric sequences, subsequently occurring with repeated nonreferential terms, such as preverbs and verbs, as in I. 91.18a *sáṃ te páyāṅsi sám u yantu vā́jāḥ* 'Let the milks unite for thee and the booties unite' and II. 35.15ab *áyāṅsam agne sukṣitíṃ jánāya / áyāṅsam u maghávadbhyaḥ suvṛktím* 'O Agni, I have extended secure dwelling to the folk, and I have extended a hymn to the liberal ones'. Again, such sequences are originally asyndetic, but the particle is susceptible to reanalysis as a conjunction, whence its further use as a sentential conjunction within noniterative, noncoreferential sequences, e.g. V. 83.

10a *ávarṣīr varṣám úd u ṣṹ gṛbhāya* 'Thou hast rained the rain. And do thou make it cease' and III. 5.3cd *ā́ haryató yajatáḥ sā́nv asthād / ábhūd u vípro hávyo matīnā́m* 'The one worth longing for, worthy of worship has climbed upon the back (of the altar). And the poet, the one to be called upon with thoughts, has come into existence'.[5]

Once we perceive the role of anaphora as an exponent of discourse cohesion, we can more easily understand the seemingly pleonastic usage of the *sá/tá-* pronoun in initial position within clauses having second person verbs. We note the following examples:

III. 19.5a-c *yát tvā hótāram anájan . . . / . . . devā́ḥ // sá tvā́m no agne (a)vitéhá bodhi* 'Since the heavenly ones have anointed thee Hotar . . . As such, do thou become an aider for us here, O Agni.'

VI. 21.7cd.8ab *táva pratnéna yújyena sákhyā / vájreṇa dhṛṣṇo ápa tā́ nudasva /// sá tú śrudhīndra nū́tanasya / brahmaṇyatáḥ* . . . 'Thrust off those (demonic apparitions), O bold one, with thy ancient ally, thy friend, the cudgel. / Do thou hearken, O Indra, unto the current utterer of the formulation . . .'

VI. 64.6 *sugótá te supáthā párvateṣv / avāté apás tarasi svabhāno // sā́ na ā́ vaha . . . / rayíṃ dívo duhitaḥ* . . . 'Good ways and good paths are thine among the mountains. (Even) in a place without wind thou crossest the waters, O self-radiant one. As such, convey hither to us . . . wealth, O daughter of heaven . . .'

In each of the above passages involving, respectively, an overt second person pronoun (*sá tvám*), an archaic second person pronoun functioning synchronically as a particle (*sá tú*, cf. Klein 1982: 9–11), or no overt second person pronoun (*sā́ na ā́ vaha*), the synchronic function of *sá* is to facilitate discourse cohesion by providing or reinforcing anaphoric coreference. Recalling the statement of Apollonius that second person pronouns are nonanaphoric, we may interpret *sá* in these passages as rendering an inherently nonanaphoric person explicitly anaphoric.

2. The foregoing discussion has centered on the role of anaphora as an exponent of textual cohesion in the absence of overt conjunction. However, anaphora plays an important role as a cohesive operator in constructions where overt conjunction is also found. In the remainder of this paper we shall explore some of the interrelationships between anaphora and conjunction in the Rigveda.

One of the most frequent tropes employed by the Rigvedic bards is relativization. Although the relative pronoun in the Rigveda frequently possesses a specifying or individualizing value (cf. Gonda 1954), relativization is often purely stylistic, as can be seen by a comparison of VIII. 63.6b *kṛtā́ni kártvāni ca* 'the things which have been done and are to be done' and I. 25.11c *kṛtā́ni yā́ ca kártvā* 'id.'. Such relativization serves to facilitate the creation of the fundamental discourse strategies *yá- . . . sá/tá-* and *yá- . . .* noun together with their inverted variants, as noted above. Beside the type X *yá- ca* Y we also find a syntagm *yá-* X *yá- ca* Y. That this trope need not represent a true relativization of the conjoined members can be seen from a passage such as I. 101.6a *yáḥ śū́rebhir hávyo yáś ca bhīrúbhiḥ* 'Who is to be called by the heroes and by the cowardly', where the conjoined terms are *śū́rebhiḥ* and *bhīrúbhiḥ*, neither of which is syntactically relativized. Rather, by an optional stylistic transformation which we shall designate anaphorization, each of these terms is made into an argument of a relative clause. Schematically, **yáḥ śū́rebhiḥ . . . bhīrúbhiś ca → yáḥ śū́rebhiḥ . . . yáś ca bhīrúbhiḥ*. The result is stylistic anaphora together with ex-

plicit conjunction. Moreover, within the larger framework of the full stanza in which this pāda occurs we find anaphora qua coreference at a higher level of discourse structure (*yáḥ . . . yáś ca / yáḥ . . . yáś ca // índram . . . / . . . havāmahe*, an expansion of the basic coreferential type *yá- . . .* noun). At the same time, pādas a and b constitute a perfect vertical isosyllabic responsio (for the terms, cf. Gonda 1959: 166–176), itself a type of anaphora involving complete syntactic constructions:[6]

I. 101.6 *yáḥ śū́rebhir hávyo yáś ca bhīrúbhir / yó dhā́vadbhir hūyáte yáś ca jigyúbhiḥ // índram . . . / marútvantaṃ sakhyā́ya havāmahe* 'Which one is to be invoked by heroes and by the cowardly, who is called by the fleeing and by the victorious, Indra . . . do we call for friendship together with the Maruts.'

The process of anaphorization just described is not limited to stylistic relativization. In particular, it includes the anaphoric use of conjunctions themselves. Thus, we find such Rigvedic syntagms as X *ca* Y *ca* (frequent) and *utá* X *utá* Y (infrequent), e.g. I. 25.20b *diváś ca gmáś ca* 'of heaven and of earth' and VII. 98.2c *utá hṛdótá mánasā* 'with heart and with mind'. The issue concerning the diachronic relationship between the type with single and double conjunction (cf. Dunkel 1982 with literature on *ca*) is thus a pseudo-problem. The latter is derived from the former by the universal principle of stylistic anaphora. We are not speaking here so much of a real diachronic priority as of a simple implication: given X Y *ca* and X *utá* Y, the possibility of both X *ca* Y *ca* and *utá* X *utá* Y is immediate.[7]

In the passages discussed so far, anaphora works in concert with overt conjunction in order to reinforce the conjoined structures (*yá-* X *yá- ca* Y, X *ca* Y *ca*). In many other instances, however, anaphora and overt conjunction operate at different levels of structure, with anaphora usually serving as a higher level discourse operator. This interplay of the two processes is particularly clear at VII.92.3cd *ní no rayíṃ subhójasaṃ yuvasva / ní vīráṃ gávyam áśvyaṃ ca rā́dhaḥ* 'Grant to us wealth bringing good enjoyment, a male (offspring), (and) success consisting of cows and horses'. Here we find a two-tiered cohesive structure. The anaphora *ní . . . ní* works in harmony with the metrical structure to demarcate the objects of *ní yuvasva: rayíṃ* in pāda c and the asyndetic *vīrám . . . rā́dhaḥ* in d. The overt conjunction, on the other hand, conjoins only the adjectival modifiers of *rā́dhaḥ* (*gávyam áśvyaṃ ca*), which themselves illustrate morphological anaphora (both are denominal adjectives in *-ya*) and homoioteleuton. A three-tiered anaphoric structure is seen in the following passage:

I. 112.23 *yā́bhiḥ kútsam ārjuneyáṃ śatakratū / prá turvī́tim prá ca dabhī́tim ā́vatam // yā́bhir dhvasántim puruṣántim ā́vataṃ / tā́bhir ū ṣú ūtíbhir aśvinā́ gatam* 'With which (aids) you two aided Kutsa, the son of Arjuna, you possessors of a hundred determinations, Turvīti and Dabhīti, with which you aided Dhvasanti (and) Puruṣanti, with those (very) aids, O Aśvins, come hither.'

Within this passage the tightest cohesions, reinforced by both rhyme and meter, are *prá turvī́tim prá ca dabhī́tim* and *dhvasántim puruṣántim*, the former accompanied by stylistic anaphora plus *ca*, the latter asyndetic. At a higher level, the outer anaphora *yā́bhiḥ . . . ā́vatam // yā́bhiḥ . . . ā́vatam* defines the enclosed structures as hierarchically equivalent. Here the anaphora is not merely lexical, but also syntactic: a constructional pattern is repeated. The entire stanza is then defined as a unit by the coreferentially anaphoric structure *yā́bhiḥ . . . tā́bhir ū ṣú*.

A pattern showing both overt conjunction and lexical anaphora occurring at different levels of structure is seen in the following instance:

II. 13.7 *yáḥ puṣpíṇīś ca prasvàś ca dhármaṇā / ádhi dáne vy àvánīr ádhārayaḥ // yáś cā́samā ájano didyúto diváḥ / . . . sā́sy ukthyàḥ* 'Who by reason of duty hast distributed the blossoming and fruitbearing (plants) (and) the streams over the meadow, and who hast begotten the incomparable lightnings of heaven . . . as such, thou art praiseworthy.'

In this passage the overtly conjunctive anaphorization *puṣpíṇīś ca prasvàś ca* forms a tightly cohesive unit on both the syntactic and semantic level. To this is then appended the asyndetic *avánīḥ* of pāda b. This entire conjoined structure, occurring only within the initial distich (X *ca* Y *ca* / Z), functions as a constituent within the larger conjoined structure signalled by the outer anaphora (both lexical and referential) *yáḥ . . . yáś ca*, which informs each distich. Typically, the entire structure is rendered cohesive at the outermost level of the stanza by the referentially anaphoric sequence *yáḥ . . . yáḥ . . . sá.*

Although the cohesive function of anaphora in both its senses has been illustrated with evidence from the Rigveda, we are dealing with a universal principle of rhetorical style, both poetic and prosaic. The interrelationships between the two types of anaphora are particularly clear in the Rigveda, owing to the importance of the *yá- . . . sá/tá-* structure in this text, with its possibilities for expansion (e.g. *yá- . . . yá- . . . sá/tá-, yá- . . . sá/tá- . . . sá/tá-*, etc.), as well as to the frequent repetition of preverbs, negative particles, other adverbials, and verbs. These figures are of great interest for the way in which they complement other 'outils de structuration' within the Rigveda, specifically the rich system of conjunctions: (primarily) word-level *ca*, generally coordinating *utá*, originally anaphoric *u*, clausal and interstanzaic *ádha* and *áthā*, etc. (cf. Klein, 1978b, 1980, 1985). To these structural signals we may add the strong tendency for the major Rigvedic metrical units (the pāda, distich, and stanza) to coincide with important units of thought or discourse (cf. Migron 1985). Taken together, these factors render the discourse structure of the Rigveda, for all the complexities offered by the meaning of the text itself, in some sense simpler and more transparent than that of other archaic Indo-European poetic texts.

Notes

1 Dionysius seems not to have considered interrogatives, indefinites, and anaphorics to be true pronouns, since when discussing the pronoun (one of his eight parts of speech) he refers only to personal, possessive, relative, and reflexive forms.

2 Although this treatise is traditionally ascribed to Demetrius of Phaleron, an Athenian peripatetic philosopher and statesman of the late fourth century B.C., Roberts concludes, on the basis of both internal and external evidence, that it is more likely to have been composed in either the first century B.C. or the first century A.D., "the latter period being on the whole the more likely" (1902: 64).

3 Wackernagel (1928: 84) states that "Windisch hat das Verdienst in seiner Abhandlung über das Relativpronomen . . . diese Unterscheidung [sc. anaphoric vs. deictic] samt dem Terminus 'anaphorisch' wieder ans Licht gezogen".

4 For the relationship between the terms 'annunciatory' and 'cataphoric' cf. Klein, 1985 part 1: 17.

5 In her review of my book on *u* (Klein 1978a), Madame Bader (1979) rightly emphasizes the unitary nature of anaphora in both its senses, hence the unitary function of *u*. However, she goes too far, in my view, in rejecting the anteriority of the particle's coreferential function relative to its conjunctive value. The crucial point is that coreferential *yá- . . . sá/tá- u* cannot be interpreted as conjunctive (*'which one . . . and that one'), whereas coreferential-iterative *tám . . . tám u* can be so viewed ('that one . . . and that one'). See now Klein ms. for a parallel development in the diachronic syntax

of the cognate Homeric Greek particle *aŭ*. For another instance in the Rigveda where surface-level ambiguity serves as the catalyst for syntactic change, see Klein 1982.

6 It is important to note that anaphora in the larger sense is not limited to lexical repetition, but may include iteration at any linguistic level. Thus, morphological iteration and its frequently associated rhyme or (when it involves contiguous terms) homoioteleuton (e.g. VII. 63.1c *mitrásya váruṇasya*) may be considered a subtype of anaphora in which the repeated stretch is limited to only a part of a lexical item. The same may be said of syntactic repetition or structural parallelism, where the repeated matter involves an entire word group. I. 101.6ab provides examples of anaphora occurring simultaneously at all these levels.

7 Since the type X *ca* Y *ca* is reconstructible for Proto-Into-European, it probably provides support for the rare *utá* X *utá* Y (10 times, including the polysyndetic *utá* X_1 *utá* X_2 . . . *utá* X_n). The latter syntagm illustrates the appropriate position for a pre-positional conjunction, the former, for a post-positional conjunction.

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The importance of Saussure's 'Mémoire' in the development of historical linguistics

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0.0 Introductory Observations

Recent translations into Russian (1977) and Italian (1978) of Ferdinand de Saussure's *Mémoire sur le système primitif des voyelles dans les langues indo-européennes*, which had first appeared in Leipzig in December 1878 (Saussure 1879), may be taken as an indication of the continuing interest in and appreciation of Saussure's contribution to comparative-historical Indo-European linguistics.* I do not know of any other book in linguistics of the last quarter of the 19th century that has been translated in recent years, with the exception of Hermann Paul's *Prinzipien der Sprachgeschichte* of 1880 (5th revised edition, 1920), which had translations into Russian and Japanese in 1960 and 1965, respectively. But Paul's book was a contribution to general linguistics rather than historical linguistics as the title of his book may suggest, and as such it was much less quickly outdated as is common in a field in which new discoveries and advances make yesterday's findings obsolete tomorrow. The fact that Saussure's *Mémoire* did not share the fate of most other studies of the period calls for special reasons, and I am hoping that the present paper will offer at least a few of these.

0.1. Early reactions to Saussure's 'Mémoire'

There are indications that Saussure's 300-page study on the sound system of Proto-Indo-European made a notable impact on linguistic circles of the time, though perhaps not the impact that the young author might have hoped for. We may recall that when the book was published, Saussure was just 21 years old, and a student of linguistics at the University of Leipzig for barely four semesters; indeed, when he defended his thesis on the absolute genitive in Sanskrit in February 1880, it is said that one of his examiners assumed that he was a young relative of the well-known author of the *Mémoire*! But apart from Saussure's precocity, there were other factors that hampered the immediate success and the acceptance of the ideas he proposed in the *Mémoire*. Despite the fact that he was a student of the Junggrammatiker, notably Karl Brugmann (1849–1919), the prime mover of this group of

* Revised version of a paper first presented at the Sixth International Conference on Historical Linguistics held in Poznań, Poland, on 22–26 August 1983. It is with distinct pleasure that I dedicate it to Henry M. Hoenigswald as a token of my admiration for his scholarship as an historical linguist as well as an historian of linguistics.

young revolutionaries in historical-comparative linguistics at the time, Brugmann did not concur with the main tenets of Saussure's argument, although he was impressed by his breadth of coverage, circumspection, and "nicht gewöhnliche Combinationsgabe". Indeed, Brugmann (1879a: 774) felt that Saussure had proposed a purely aprioristic scheme ("rein aprioristische Construction"), which did not hold water, and he was not convinced that Saussure's proposals would have to lead him to a substantial revision of his own views (cf. also Brugmann (1879b: 261–262), and the relevant quotations in Redard (1978: 33)).

Hermann Osthoff (1847–1909), Brugmann's close collaborator during the 1870s and 1880s, expressed himself in a much more hostile manner to Saussure's theories in several articles published in volumes 2 and 4 of *Morphologische Untersuchungen* in 1879 and 1881, qualifying them as a 'total failure', 'radical error', and the like (cf. Redard (1978: 35) for details).[1] Gustav Meyer (1850–1900), another member of the group, incorporated part of Saussure's findings in his *Griechische Grammatik* of 1880 (3rd ed. 1896), without ever mentioning the source of the series of the Indo-European ablaut he was basing his work on. In short, it can be said that, at least in its initial phase, Saussure's *Mémoire* did not find the reception within the immediate circle of the Leipzig linguists one might perhaps have expected. However, other reviewers of the book who were either opposed to the Young Turks at Leipzig or abroad and thus far removed from the scientific quarrels among German scholars, seem to have agreed on at least one trait of Saussure's *Mémoire*, namely, that it was theoretically very demanding, that the author had a penchant for algebraic formulae, and that the argument was difficult to follow because of a predilection for abstraction. Thus August Fick (1833–1916), the head of the Göttingen linguistic circle which included, inter alia, Hermann Collitz (1855–1935), reviewing Saussure's book at considerable length, noted Saussure's fondness of "mathematische Formulierungen" (1880: 420), but concluded, after having criticized a number of points of detail in Saussure's argument:

"Die etwas künstliche Anordnung des Stoffs und eine eigentümliche Vorliebe des Verf[assers] für mathematische Formulierungen erschweren das Studium des sonst klar geschriebenen Werkes, wer aber die Mühe nicht scheut, wird sich durch mannigfache Belehrung und Anregung reichlich entschädigt finden." (Fick 1880: 239)

The Celtologist, (Sir) John Rhys (1840–1915), writing for a more general audience in a British monthly, likewise noted, in his very positive review:

"The work is so technical and the reasoning so complicated that it is very difficult to give any idea of it excepting to those who have had a thorough training in Aryan glottology [i.e., Indo-European linguistics]; but it may at once be said that it is the most important and epoch-making work that has appeared since [Johannes] Schmidt's [(1843–1901)] [*Zur Geschichte des indogermanischen*] *Vocalismus* [2 vols., Weimar: H. Böhlau, 1871–75] was published." (Rhys 1879: 234)

We may also cite a similar opinion by the French Latinist and comparative linguist Louis Havet (1849–1925), who in fact wrote the most extensive review of Saussure's *Mémoire*, but had similar misgivings, arguing that it had "un defaut grave: il est extraordinairement dur à lire". Havet found that Saussure was abusing abstract designations and was engaging in an excessive use of (at times newly coined) technical terms, concluding his complaints by stating that "Ainsi, M. de Saussure fait suer sang et eau à ceux qui le lisent" (Havet 1978 [1879]: 107). However, apart from these criticisms, Havet's review of Saussure's book was very favourable indeed, leading to a life-long friendship and correspondence between the two scholars (cf. Redard 1976).

There is still at least another review of the *Mémoire* that deserves being mentioned, namely, the one by the young Polish linguist Mikołaj Kruszewski (1851–87), which appeared in Russian in 1880. It also included an account of Brugmann's "Nasalis sonans" article of 1876, but it is obvious that Saussure's *Mémoire* was far more attractive to the theoretically inclined Kruszewski, who regarded Saussure's book as an important contribution to the method of phonology, and in fact as marking a new phase in the study of Indo-European phonology (Kruszewski 1978 [1880]: 444 and 450, respectively). (I may also mention in parenthesis that the *Mémoire* inspired Kruszewski's work in various ways; apart from leading him to study questions of ablaut in Indo-European, especially in Slavic, Kruszewski – and his mentor and collaborator Baudouin de Courtenay – took from the *Mémoire* a number of concepts and terms, such as 'phoneme', 'alternation', and 'zero'. Indeed, Kruszewski translated 'phoneme' into Russian as 'fonema' and proposed to use it as a phonological term in contradistinction to 'zvuk' "sound", a distinction which made history in linguistics.)

In sum, we may say that outside the narrow circle of the Leipzigers Saussure's work was well received when it was first published. It is true that in France Saussure's proposals found an untiring critic in Paul Regnaud (1838–1910), a professor of Classics and comparative grammar at the University of Lyon (e.g., Regnaud 1889, 1890, 1891), but it seems that Regnaud's attacks on Saussure's system of the proto-Indo-European vowels were ignored by most of his contemporaries, probably for the simple reason that the younger generation of Indo-Europeanists had long since rejected the basic assumptions of August Schleicher (1821–1868) in matters of Proto-Indo-European phonology, to which Regnaud still subscribed.

0.2. The reception of Saussure's 'Mémoire' during the late 19th and early 20th century

Ignoring for the moment the peculiar reception Saussure's theories received from scholars such as Hermann Möller (1850–1923) and Albert Cuny (1869–1947) – it will concern us later in this paper – we may note that members of the next generation of Indo-Europeanists, notably Saussure's former student at the École Pratique des Hautes Études in Paris, Antoine Meillet (1866–1936), and two German scholars, Wilhelm Streitberg (1864–1925) and Hermann Hirt (1865–1936), had a particularly high opinion of the *Mémoire*. Hirt can be said to have been the first scholar to fully incorporate Saussure's findings in his monograph of 1900; already in 1885, the brilliant comparative linguist Heinrich Hübschmann (1848–1908), one of Saussure's former teachers at Leipzig, was close to accepting Saussure's theory part and parcel had it not been for an error on Saussure's part in taking Lat. *agō* and the like as an aorist present of **Aǵ-* (Hübschmann 1885: 2–3, cf. Mayrhofer 1981: 26–27, n. 75). Hirt, for his part, regarded Saussure's *Mémoire* as "ein bahnbrechendes Werk, noch heute von größter Bedeutung", some two generations after its publication (Hirt 1927: XXV), thus echoing Streitberg's opinion expressed in his obituary of Saussure of 1914:

"Das Buch ist de Saussures Meisterwerk: noch heute, nach einem Menschenalter, wirken Inhalt und Form mit derselben bezwingenden Macht wie am Tage des Erscheinens – von wieviel sprachwissenschaftlichen Werken, auch solchen höchsten Ranges, kann man das Gleiche sagen?" (Streitberg 1966 [1914]: 103)

Last but not least, reference should be made to Meillet, in particular to his influential *Introduction à l'étude comparative des langues indo-européennes*, which first appeared in 1903, and which bears the inscription "A mon maître Ferdinand de Saussure à l'occasion des vingt-cinq ans depuis la publication du *Mémoire* . . . (1878–1903)", an inscription that was retained in the many subsequent editions of the text. As a matter of fact, this book remained for Meillet Saussure's major achievement, and not the posthumous *Cours de linguistique générale*, with the result that even after the latter's appearance, Meillet associated the well-known phrase (wrongly ascribed to Saussure, though undoubtedly in his spirit) that language is a system 'où tout se tient' with the *Mémoire*. (Cf. Meillet (1903; 8th edition (1937), p. IX [= 1903: X], 475), and elsewhere; and Brogyanyi 1983, for details.)

In 1887, Saussure's *Mémoire* was republished in Paris; at this time, Saussure contemplated accompanying the new edition with an attack on his detractors (cf. Redard 1978: 36). One may speculate who or what circumstances persuaded Saussure to suppress his ill feelings toward those who did neither understand nor appreciate his accomplishments. It could have been the recognition he received from scholars outside the narrow circle of the Junggrammatiker, notably Havet, Fick, and Hübschmann, all between 9 and 24 years his seniors, and perhaps by the opinion expressed by the doyen of Indo-European linguistics at Leipzig, Georg Curtius (1820–85), in a personal letter of 1884: "Lange habe ich nichts der Art gelesen, was mich so entschieden überzeugt hat."[2]

0.3. The third phase in the reception of the 'Mémoire'

The phase following Saussure's death in 1913 may well be regarded as the most important period in the development of the discussion of the Proto-Indo-European phonological system, a phase which has not yet come to a close. Saussure's *Mémoire* may be said to have played an important role in this discussion, which in fact began as early as in 1879, though more as a fairly weak undercurrent as we may see later on. There were a number of reasons for the slow posthumous recognition of Saussure's work in Indo-European linguistics. For example the *Cours* of 1916, owing to its seeming anti-historical bias, tended to detract from the fact that Saussure's life-time contribution lay almost exclusively within diachronic linguistics. Also the hostile feelings toward anything German of the period during and following the First World War made the work of a French Swiss scholar rather attractive as it appeared to oppose the traditional kind of linguistics widely associated with Germany. As a result, among many of the new generation of linguists historical linguistics became a side-issue and the preoccupation of a comparatively small number of experts. This development is clearly reflected in most histories of linguistics. If they mention Saussure's *Mémoire* at all, they present it as something of marginal interest, focussing instead almost their entire attention on the work that in fact did not have his imprimatur and, as we have come to understand in recent years, does not reflect his true intentions. In short, it has become customary to refer to a Saussure who wrote the *Mémoire* in his youth, and another who is the author of the *Cours*, which brought about a revolution in modern linguistics, with the result that even the large portions devoted to diachronic linguistics it contains are patently ignored.

It would exceed the frame of the present paper to show, as Cristina Vallini (1969: 49ff) has already done to some extent, that in fact there is no justification for the traditional

conception of the existence of 'deux Saussure' (cf. Redard 1978), but that the early work is consistent with his later, largely unpublished theories. Perhaps it should be stated in the present context that the critical edition of the *Cours*, carefully compiled by Rudolf Engler, contradicts affirmations in the text as edited by Bally and Sechehaye, including those frequently attacked ones according to which synchrony and diachrony are supposed to be regarded as two subjects apart: Nothing could have been more contrary to Saussure's view on the proper relationship between these two 'points de vu'.

We know from the *Cours* the central role that the concept of 'system' has played in Saussure's linguistic theory. But if we analyse his *Mémoire* carefully, we will notice that the same notion is basic to Saussure's argument therein as well. Indeed, we may say that in the *Mémoire* 'system' is the 'clé de voûte', the key stone holding the entire edifice together. Interestingly enough, while others referred to Saussure's book as the *Mémoire* for short, he himself preferred to refer to it as his *Système des voyelles*, with a capital 'S' (cf. Watkins 1978:61). Moreover, Saussure justified the title of his book in the following terms, at the same time indicating his approach to the subject matter:

"Étudier les formes multiples sous lesquelles se manifeste ce qu'on appelle l'*a* indo-européen, tel est l'objet immédiat de cet opuscule: le reste des voyelles ne sera pris en considération qu'autant que les phénomènes relatifs à l'*a* en fourniront l'occasion. Mais si, arrivé au bout du champ ainsi circonscrit, le tableau du vocalisme indo-européen s'est modifié peu à peu sous nos yeux et que nous le voyions se grouper tout entier autour de l'*a*, prendre vis-à-vis de lui une attitude nouvelle, il est clair qu'en fait c'est le *système des voyelles dans son ensemble* qui sera entré dans le rayon de notre observation et dont le nom doit être inscrit à la première page." (Saussure 1879[1878]: 1; emphasis added: KK)

Similarly, and quite revealingly I believe, Saussure ends his book with the following paragraph, in which he discusses special instances of the possible presence of this elusive Proto-Indo-European *a* or, rather, in Saussure's analysis, *e*:

"Cette inconstance de la voyelle [in western Indo-European languages] révélerait, dans d'autres circonstances, la présence du phonème A; mais si telle est la valeur de l'*e* dans *ἀFέξω*, la relation de cette forme avec *vákšati, ukšáti, αὔξω*, aussi bien que sa structure *considérée en elle-même* cessent d'être compréhensible pour nous." (Saussure 1879: 283; emphasis added: KK)[3]

This concept of system is not simply what is implied by expressions such as 'le système de Curtius' (Saussure 1879: 2), 'système de Schleicher' (1879: 3–4), and the like, which refers merely to the hypothesized inventory of the sounds of a language, but, as will become evident in the subsequent discussion, a system of relationships of phonemes. Indeed, we may discover in Saussure's *Mémoire* the source of his later definition of phonemes as "des entités oppositives, relatives et négatives" (Saussure 1916: 164; cf. Saussure 1968: 268). Already the first paragraph of his *Mémoire* cited above suggests this.

1.0. Saussure's argument in the 'Mémoire sur le système primitif des voyelles dans les langues indo-européennes'

Before delineating the manner in which the *Mémoire* made an important contribution to linguistics, both historical and theoretical, let us first present the main tenets of Saussure's argument, his approach having been signalled in the introductory paragraph of his work, namely, to investigate the various reflexes in the attested languages of what has been com-

mon Indo-European *a*, with all other vowels of the system being considered only in relation to this phoneme, but with the result that eventually the entire system of Indo-European vowels is established.

Saussure proceeds in a most rigorous fashion. He discusses – and discards – one proposal after another made by Curtius, Schleicher, Fick, and others, in part also those by his teachers, including Brugmann and Osthoff. Subsequently, he adduces evidence for his own views, and this with a precision that must have astounded his contemporaries, and which impresses us still today. I am quoting just one example from the English translation, provided by Winfred P. Lehmann in 1967, to illustrate Saussure's style of scientific discourse. In this passage from chapter 2 of his *Mémoire* in which he proposes to separate the 'old' from the 'true', by lifting "tout l'humus moderne que différents accidents avaient amassés sur lui", Saussure, after having established the various correspondences between root vowels found in what he terms the northern languages, e.g., Germanic and Slavic, and the southern languages, such as Latin and Greek, proceeds as follows:

"How then could the *a* and *o* of the languages of the South have arisen from one and the same original *a*? By what miracle could this old *a* be coloured to *o, and never to a*, precisely in all the times that it is found in the company of an *e*? – Conclusion: the dualism of *a* and *o* is original, and it must be that in the single *a* of the North two phonemes were confused.

Confirmation: when a root contains *a* in Greek or Latin, and when this root is found in the languages of the North, one observes in the first place *that this a never alternates with e*, as is the case when the Greek replies by an *o*. Thus Goth. *vagja* = Gk. *okhéō, hlaf* = Gk. *(ké)klopha* are accompanied by *viga* and *hlifa*. But *agis(a-)* = Gk. *ákhos*, or *ala* = Lat. *alo* do not have a parent form with *e*. On the other hand, the roots of the latter type have one characteristic, unknown to the first type: the ability to lengthen their *a* (*agis: ōg, ala: ōl*), of which we will have to take account below.

Brugmann has designated with a_1 the prototype of the European *e*; his a_2 is the phoneme which we have called *o* up to now. As to this third phoneme which is the Greco-Italic *a* and which constitutes one half of the *a*'s of the languages of the North, we will designate it by the letter *A*, after noting well that it is neither the *e*(a_1) nor the *o*(a_2). – Excluding for the time being the other possible kinds of *a*, one obtains the following table:[4]

Langs. of the North	*Primordial state*	*Greco-Italic*
e	a_1	e
{	a_2	o
a	A	a

At first sight, it may appear to be an over-interpretation to claim that Saussure's use of 'state' ("état") in this table has any significance. But if one follows his argument more closely, it becomes evident that he is primarily concerned with establishing an 'état de langue' in his *Mémoire*, namely that of the Proto-Indo-European vocalic system, not its succession. Indeed, one indication of this intention is Saussure's regular use of the term 'phonème' in his work instead of 'son', a usage which is the first of its kind in the history of linguistics, even though the term itself had not been invented by Saussure, as Hjelmslev (1970: 125) holds. Saussure wanted to signal by this that he is not concerned with matters of phonetic realization; as a matter of fact, he rarely makes a reference to phonetic detail in his book (cf. Polomé 1965: 10, and n.7), preferring instead an algebraic notation devoid of phonic substance. From what will become clear below, we may say that Saussure's

Mémoire is structuralist in approach, and that it constitutes in essence a contribution to structural linguistics 'avant la lettre' and not merely to historical-comparative linguistics as it is commonly understood.

However, this understanding of the *Mémoire* is of a much later date, and it required the work of kindred spirits such as Louis Hjelmslev (1899–1965) to fully appreciate this endeavour on Saussure's part (cf. Hjelmslev 1970: 123–128, for details). In the context of the present paper this essential trait of Saussure's argument must remain a matter of lesser consideration, although it should be clear that his theory of Proto-Into-European ablaut cannot be understood without the concept of language as a rigorously organized system underlying it (cf. also Schmitt-Brandt 1967: 2; Vincenzi 1976). For the field of Historical Linguistics, which at the time was almost identical with linguistics *tout court*, Saussure's *Mémoire* must now be regarded as introducing a revolution, even though it is safe to speak of 'une victoire retardée' or 'tardive', since, as we have seen in the introduction (0.1.–0.2. above), most of Saussure's contemporaries did not grasp the full implications of his theories, and a number of distinguished scholars, including Brugmann, did not see the necessity of revising their inventory of the Proto-Indo-European sounds in accordance with Saussure's findings. As will become clear from what follows, however, there were, apart from Saussure's quasi-algebraic approach to the subject, other reasons which explain the difficulty Saussure's contemporaries, not least the Junggrammatiker with whom he had been associated for some time, experienced in accepting his proposals.

1.1. Saussure's revisions of the Proto-Indo-European vocalic system

We have already hinted (1.0. above) at Saussure's general procedure in dealing with the problem of the Proto-Indo-European vocalic system. However, we cannot fully appreciate his in fact audacious innovations unless we present ourselves what had been, until the mid-1870s, received opinion.

Until about that period, Indo-European linguistics, despite a number of statements by their proponents to the contrary, was essentially 'Sanskrito-centric' (cf. Mayrhofer 1983: 130–136 passim). This meant that, in matters of phonology especially, linguists clung to the belief that Sanskrit, which, until the decipherment of Hittite during the early decades of the 20th century, supplied the comparative linguist with the oldest data, was in effect reflecting the earliest state of Indo-European. As a result, the inventory of Proto-Indo-European vowels was regarded as consisting of three vowels only, which could either be long or short:

i u

a

Since Greek and Latin for instance displayed a five-vowel system, this was accordingly interpreted as representing an innovation on the part of these (classical) European languages, usually regarded as a degeneration of the primordial state; cf. the following correspondences taken from Arlotto (1972: 118):

Sanskrit	Greek	Latin	Indo-European
(a) *ad-mi* 'I eat'	(e) *ed-omai* 'I will eat'	(e) *ed-ō* 'I eat'	*(*e) *ed-* 'to eat'
(ā) *prā-ta* 'full'	(ē) *plē-rēs* 'full'	(ē) *plē-nus* 'full'	*(*ē) *plē-* 'to fill'
(a) *aṣṭā* 'eight'	(o) *oktō* 'eight'	(o) *octō* 'eight'	*(*o) *oktō* 'eight'

and so on, where there are still two other correspondences in Greek and Latin to Sanskrit *a* and *ā*, which are also *a* and *ā*, respectively. The i/u/a vowel triad, however, had been codified in Schleicher (1861: 134–135), and was widely accepted for several years after Schleicher's death in 1868. Indeed, his work was reedited twice (1871 and 1876) by two of his former students, Johannes Schmidt (1843–1901) and August Leskien (1840–1916), and thus continued its influence in the teaching of Indo-European phonology. The rejection of the Proto-Indo-European vowel triad and the acceptance of a much more variegated system of both short and long vowels in Indo-European was one of the main accomplishments of the younger generation of comparative-historical linguists during the later 1870s. But Saussure was not satisfied with a simple enlargement of the vowel triad to a five-vowel system as the classical languages may have suggested, namely:

i u
e o
a

However, this view of the Proto-Indo-European vowel inventory had become widely accepted about the same time that the *Mémoire* appeared. Saussure did not appear to subscribe to this configuration of the vocalic system of Indo-European and, as a result, made it difficult for his contemporaries to appreciate and, furthermore, to accept his own proposals. As a matter of fact, these came, as Szemerényi (1980: 115) notes, at the worst possible time.

Without adducing much evidence for his hypothesis, Saussure claimed (1879: 135) the following: First of all, the basic root vowel in Proto-Indo-European is /e/ rather than /a/ or a variety of vowels; this was the a_1 that we noted earlier, and which both in the northern dialects and Greco-Italic are represented by *e* (cf. Saussure 1879: 52). He had earlier noted that the high vowels *i* and *u*, like the nasals and liquids, could take on (semi-)vocalic function, and therefore (1879: 8) he put them together in one group as "coefficients sonantiques": Depending on the presence or absence of the *e* within the root, *r l m n* become sonants, and *i* and *u* take on consonantal function.[5] (We are long since used to calling these sound 'resonants'.) In other words, Saussure argued that all root vowels consist of combinations between the basic Proto-Indo-European *e* and a resonant or 'sonant coefficient'. Under certain conditions, this *e* was ejected (quantitative ablaut), leading the resonants to take on syllabic function, and under others, it alternated with *o* (qualitative ablaut), represented by a_2 in earlier theories (such as those by Brugmann).

In addition to alternation with *o*, *e* could simply disappear altogether, thus producing the following results at the "degré zéro" ('zero grade'); example:

**peik-* > **pik-* **penk-* > **pṇk-* and **pet-* > **pt-* (without a vowel)

In addition to the six sonorants or resonants mentioned above, however, Saussure hypothesized the presence of two more "coefficients sonantiques", which he symbolized as *A* and *O* respectively, both sounds which could appear on their own only at the so-called zero grade of a root form, and would produce *a* and *o* respectively – the latter being of course different from the o_1 in the ablaut relation (and should therefore be identified as o_2) or – and this should be highlighted here: shwa. Saussure (1879: 178) described the latter as a "voyelle indéterminée" and as "une *espèce d'e muet, provenant de l'altération des phonèmes A et O*" (italics in the original).

These two added sonant coefficients play a central role in Saussure's argument, as he sets up the following combinations to obtain the series of long vowels:

e + A = ē and ā, and e + O = ō

As Saussure does not specify the phonetic quality of his *A*, this 'sonant coefficient' can produce both *ē* and *ā; ō* can also be the result of the apophonic *o* + *A* or *o* + *O*. At any rate, all long vowels are products of the original *e* plus a coefficient, and hence not original as having independent existence. Examples:

**bheA-* = **bhā-* cf. Gk *phā-mi* (Attic Gk: *phēmi*) 'I say'
**bhA-* (zero grade) cf. Gk *phă-mén* 'we say'
**deO-* cf. Gk *dō-* (root of) 'to give'
**dO-* (zero grade) cf. Gk *do-tós* 'given'

No doubt, Saussure operates with what we nowadays refer to as 'underlying forms', deriving the actual attested forms through specific rules. By the same method, Saussure (1879: 248) sets up the rule "*Le groupe* sonante + *A*, *précédé ou placé au commencement du mot, se change en* sonante longue, *quel que soit le phonème qui suit*" (italics in the original), so that *ī* and *ū* as well as the long sonorants ṇ̄ ṃ̄ l̥̄ r̥̄ are derived from *iA*, *uA*, *ṇA*, *r̥A*, and so on, or, in notation suggested by Saussure only in 1891 (cf. Saussure 1922: 603), sonant plus shwa. In effect, it would seem that Saussure was the first abstract phonologist in that he was operating with hypothetical constructs and indirect (distributional) evidence.

From his *Mémoire* at least, it is quite clear that the shwa was supposed to have vocalic rather than consonantal quality,[6] and Mayrhofer (1983: 144n.91) may be justified in his hesitation to accept the view expressed by others that in his 1891 observations Saussure was in fact modifying his earlier interpretation. This question is relevant in the subsequent discussion of Proto-Indo-European phonology (cf. 1.2. below); but what is important to emphasize in the present context is that Saussure had developed a fairly abstract and rigid system of phoneme combinations which allowed him to produce the various attested forms in individual Indo-European languages. As Szemerényi (1980: 115) has pointed out, Saussure did not present much in terms of evidence for his theory except for hinting at such incidents in Greek and Sanskrit which may be explained by his assumptions. The beauty of the matter is that much of what Saussure advanced on theoretical and structural grounds was subsequently shown to be correct, to the extent that we may be right to say that with Saussure's *Mémoire* a new phase in the history of Indo-European phonology had been set in motion.

1.2. Saussure's contribution to the laryngeal theory

It is not surprising that Saussure's discussion of Proto-Indo-European vowels contained one or the other error, though none substantial enough to lead to a rejection of his theory as a whole. We have already hinted at the fact that Saussure was not much concerned with phonetic detail in his argument; this may have indeed been a point of strength rather than weakness, as some modern phonologists may well be ready to admit. But in Saussure's time, when linguists were beginning to take note of articulatory phonetics, this attitude might easily have been subject to criticism. August Fick, in his review of the *Mémoire*, was one of the first to point to the difficulty of deriving both *ā* and *ē* from *e*+*A*; I guess we would today call Saussure's hypothesis 'counter-intuitive' on this issue and lacking in sym-

metry. Saussure had considered such a combination of a_1a_1*(ee)* "parallèle aux combinaisons a_1A, a_1i, a_1n, [i.e., **eA, ei, en*] etc.", but he argued in fact that this would lead to 'contre-sens' (1879: 141). Fick (1880: 437), however, felt, although he conceded that his "Begeisterung für das Analogieprincip nur schwach ist" (1880: 436), that a 'coefficient sonantique' *E* parallel to Saussure's *A* and *O* was indeed desirable, at least as long as one admitted the original existence of an unaccentuated *e* in addition to Saussure's 'fundamental e'.

But as we noted in the introductory part of this paper (0.1.–0.3.), most Indo-Europeanists of the last quarter of the 19th and the first quarter of the 20th century ignored Saussure's theory of sonant coefficients, and it was only some fifty years after the publication of the *Mémoire* (and 14 years after his death) that the significance of Saussure's proposals was fully recognized. The path of recognition was long and slow, and the fact that the first scholars to see the light were outside the mainstream of Indo-European linguistics did not help to further it.

Shortly after the appearance of Saussure's book, the Danish Nordist Hermann Möller (1850–1923), who also worked in Semitic and in fact adhered to the Indo-European-Semitic hypothesis so ardently combatted by Schleicher and dismissed by the generation of Indo-Europeanists following him, proposed, in a review of a book by Friedrich Kluge (1856–1926)

„ein consonantisches element . . . , welches die eigenschaft hatte, ein vorangehendes oder (im anlaut) folgendes a_1 (das in der letzten zeit der grundsprache ein *ä* gewesen sein wird) in reines *a* zu verwandeln, und das mit vorhergehendem vocal *a* zu reinem langem *ā* zusammenschmolz." (Möller 1879: 150).

In the same review, Möller suggested that in order to justify the alternation in Gk $\theta\eta$-/$\theta\epsilon$-, it would be required that a third sound, *E*, should be added to Saussure's sonant coefficients *A* and *O* (Möller 1879: 151), n.1), quite in line with Fick's suggestions mentioned earlier. However, in contrast to Fick (1880: 438), for whom these coefficients were "von Haus aus Vocale", Möller had hypothesized them to be consonantal in nature. Henry Sweet (1845–1912), also reviewing Kluge's (1879) book, though together with Saussure's *Mémoire*, proposed by following Möller's lead to regard *A* as a voiced glottal trill, with *E* being characterized by a palatalized, and *O* by a velarized articulation (Sweet 1880: 161). In the meantime, Möller was shifting his position: While he had hypothesized "A [as] die tönende, E die tonlose kehlkopfspirans?, O das kehlkopf-*r*?" in 1879 (Möller 1879: 151 n.1), he connected the Indo-European sounds in question with Semitic in the following year by suggesting that we have to do with "wahrscheinlich gutturale von der art des semitischen, *A* = ālef, der tonlose verschlußlaut, und E wahrscheinlich der entsprechende tönende verschlußlaut" (Möller 1880: 492 n.2).

However, Möller did not draw the conclusions from his own observations for almost forty years (cf. Möller 1917: 4–5 and 53), and since his 94-page monograph on the laryngeal consonants of Indo-European and Semitic was not regarded as sound in scholarship with regard to much of the data presented therein to bolster the claim for an early Indo-European-Hamito-Semitic genetic relationship, Möller's proposals remained largely ignored. (For a detailed analysis of Möller's suggestions, see Szemerényi 1973: 5–8.) Eventually, his laryngeal hypothesis of 1879 found adherents, though not before the early years of the 20th century. Thus in 1907 his former student Holger Pedersen (1867–1953), at that time

sympathetic to the Semitic-Indo-European hypothesis, tried to adduce evidence that Proto-Indo-European shwa[7] derived from a consonant.[8] By that time, Möller (1906) had proposed, on the basis of Semitic, a theory of altogether five similar sounds, which, in the preface to his Comparative Semitic-Indo-European Dictionary (Möller 1911: VI), he called 'laryngeals', in fact stating that "die von F. de Saussure für das Vorindogermanische erschlossenen 'phonèmes' entsprechen den semitischen Laryngalen". One may wonder whether this remark and related observations by Möller led the French scholar Albert Cuny (1869–1947), another strong believer in the original genetic relationship between Afro-Asiatic and Indo-European (called 'Nostratic' by Pedersen), to assert that Saussure had, at least since 1891, thought of the shwa as having consonantal function (Cuny 1912: 119; cf. Szemerényi 1973: 8–10, for a thorough refutation of this common assertion).

But Cuny made a number of much more striking observations in his 1912 article, which followed an earlier dissatisfaction with Möller's views on the relationship between Indo-European and Semitic. Indeed, this article contains a series of important observations, of which only few will be mentioned here (cf. Szemerényi 1973: 13–15 and Jonsson 1978: 3–4 for details):

(1) The Indo-European proto-language must have contained altogether three consonantal laryngeals (E, A, O; ∂_1, ∂_2, ∂_3 or H_1, H_2, H_3, depending on notational preference) instead of Saussure's two vocalic *O* and *A*. (The series of three such 'coefficients' had been suggested as early as 1879 by Möller and 1880 by Fick.)

(2) Cuny allowed for a certain flexibility in the matter concerning the consonantal quality of these laryngeals, however. Thus he had noted that in sequences of sonorant + laryngeal + obstruent, it is always the sonorant that becomes vocalic, e.g., *sterə- : str̥tos*, which of course means that the laryngeal is less inherently vocalic than the sonorant. However, in the absence of a sonorant, the laryngeal, following the loss of a vowel, is vocalized, e.g., *sāg-* standing for (in Saussure's notation) *seAg-* has its counterpart in *səg-* (for *sāg-*), having the formula *sḀg-* underlying (Cuny 1912: 102–103). This flexibility allows Cuny (1912: 120) to show that Greek for instance had a series of three vocalic shwas.

(3) In addition to Saussure's suggestions, in particular the one made in 1891, according to which an unvoiced spirant derives frequently from *t* + laryngeal, Cuny added further evidence for the original presence of laryngeals in Indo-European, in Indic as well as in Balto-Slavic (Cuny 1912: 117–120; cf. Szemerényi 1973: 13–14).

Cuny's argument, if followed closely, reveals at least two major traits: first, he is very much indebted to Saussure's (and also to Möller's) findings; second, we may note a considerable increase in phonetic-phonological sophistication. What in addition deserves to be pointed out is that mainly internal evidence, i.e., evidence deriving from attested language material of Indo-European, was supplied for the laryngeal hypothesis. The fact that the laryngeal theory, as it is more generally called today, had originally been inspired by findings made in Semitic does not contradict this statement, though the fact that its main proponents subscribed to the pre-Indo-European genetic relationship with Hamito-Semitic, appears to have made it difficult for mainstream Indo-Europeanists to accept the theory. Therefore, further evidence was required to lead the laryngeal theory to victory, and it came from a former student of Antoine Meillet, Jerzy Kuryłowicz (1895–1978), in 1927, two generations after the publication of Saussure's *Mémoire*.

It is no exaggeration to state that Kuryłowicz's connection between Indo-European shwa and a sound actually found in Hittite, an Indo-European language older than Greek or Sanskrit as far as the available data is concerned, which could be compared with the hypothesized Proto-Indo-European phoneme (Kuryłowicz 1927), could not have been undertaken without the work of another researcher. The evidence adduced by Kuryłowicz had not been available to either Saussure or his early followers like Möller, Cuny, or Pedersen. Indeed, it was only several years after Saussure's death in 1913 that the tablets of cuneiform inscriptions, discovered during excavations near the Turkish village of Boğaz Köy during 1905–1907, had been deciphered and identified as Indo-European, namely, by the Czech scholar Bedřich Hrozný (1879–1952) in 1915 and the following years.

Kuryłowicz (1927) had examined Hittite words whose etymological connection with other Indo-European languages such as Greek or Latin was beyond doubt. He chose examples where Saussure had predicted the original presence of a sonant coefficient, in particular the *A*, and could establish the following correspondences among others, where Hittite has the laryngeal *ḫ*:

Greek *anti*	= Hittite *ḫanti*	'in front'	<**Aanti*
Greek *arges*	= Hittite *ḫarkis*	'white'	<**Aarges*
Latin *păsco*	= Hittite *paḫsanzi*	'protect'	<**paAsk-*

The above examples prove another hypothesis made by Saussure, namely, that if (originally) the laryngeal preceded the vowel, the result is a short *a*, and a long one, if it follows. As a result, it could no longer be doubted that early Indo-European contained laryngeals at one time of its development, traces of which can be found in the various attested languages. In this way, we may say that Kuryłowicz's work vindicated Saussure's theory, which from then onwards has become more and more generally accepted by historical-comparative Indo-Europeanists.

Of course, Kuryłowicz's paper of 1927 was not a one-shot enterprise; on the contrary, as Szemerényi (1973: 15–19 and especially note 40) has shown, he published a flurry of studies to establish his proof on a very broad basis of phonological evidence himself, and a number of other scholars followed his lead in quick succession. Among them, mention should at least be made here of Emile Benveniste (1902–1976) and his important work of 1935, in particular his theory of the basic structure of the Indo-European root, and Walter Couvreur's massive monograph of 1937, which for the first time accounted for the fact that the Hittite laryngeal occured sometimes in a simple, sometimes in a double consonant. Both Benveniste and Couvreur (cf. also the latter's 1939 paper) based their argument largely on Saussure's *Mémoire*, seeking, as Möller, Pedersen, Cuny, and Kuryłowicz had done before them, to adduce further proof to the basic correctness of Saussure's theories (cf. Kodzu's (1939) review article on this subject). As Polomé in his formidable survey of the history of the laryngeal theory has shown (Polomé 1965: 14ff.), the work of Kuryłowicz, Benveniste, Couvreur and others was first met with a lack of sympathy and comprehension similar to the fate of the *Mémoire* during Saussure's life-time, namely, that the hypotheses were characterized as 'constructed', 'aprioristic', 'too audacious to be true', and the like. History appears to be repeating itself.

2.0. Concluding remarks

In this paper, it has not been my intention to trace the history of the laryngeal theory; this has been done already very competently by others (e.g., Polomé 1965; Szemerényi 1973; Jonsson 1978). Nor did I mean to indicate the role that Saussure's proposals concerning the structure of the Indo-European vowel system have played in the development of this theory, by simply suggesting that he had given the initial impetus to research undertaken by others. The reason for this is simple: I believe that Saussure had much more to offer to historical linguistics than an initial suggestion which was subsequently proved to be right, although this may be no small contribution by itself. Indeed, we all have witnessed that with the advent of the so-called Chomskyan 'paradigm' hundreds of linguists and would-be-linguists have been engaged in tossing out 'theories' almost daily, frequently withdrawing them a few weeks later themselves before others had time to refute them.

In Saussure's case we have to do with something much more profound and valuable. Apart from the fact never to be overlooked that his argument was based on a considerable amount of verifiable data, Saussure presented his theory of the Proto-Indo-European vowel system in a highly organized fashion, proceding with a step-by-step interpretation of the material together with his reasoning about the data. Most importantly, he put forward a theory arrived at not merely in a wholly deductive fashion deriving from the premise that the vowel system of any language must be a complete system of relationships, but at the same time by adducing inductively, as much as was possible at the time, anything that could lead to substantiating the veracity of his general assumption. That he produced a hypothesis of consequence was therefore not simply the strike of genius – which is true nevertheless of course, but the result of a considerable familiarity with the available data, the tools of the craft, and a particularly clear mind. This last-mentioned criterion may be the key to Saussure's success, distinguishing himself from the great majority of his contemporaries and, we may add, followers. Saussure was very conscious about this, and critical especially of the linguists of his day who evidently lacked the capacity which he regarded as vital to the advancement of the field. In an unpublished draft of a letter probably dating from the 1890s, when he was planning on writing a book on the principles of linguistic science, Saussure stated:

"Je veux malgré çela résumer quel est pour moi l'exact état des preuves, car ce que je déteste chez tous les Germains comme [aussi Holger] Pedersen, c'est la manière subreptice d'amener la preuve, et de ne jamais la formuler, comme si la preuve de leurs réflexions les dispensait de mettre à nu leur opération logique". (Quoted from Watkins [1978: 65])

What we may learn from Saussure's example for historical linguistics as well as for the study of language in general is the following:

(a) The analyst must lay bare, to himself and others, the entire procedere of his argument; his approach must be clearly presented and well reasoned.

(b) Historical-comparative linguistics is not a field of mere data gathering and ordering, a barren positivistic enterprise as has been common at least since the 1870s, but a subject which can only advance significantly if it admits the setting-up of hypotheses which must be substantiated by subsequent research.

(c) Language in general is an ordered whole; as a result, the tools of analysis must be used on the assumption of the basic systematicity of the data.

These and other ideas have become familiar to us through the *Cours de linguistique générale*, although the manner in which it was compiled has left the impression on three generations of linguists that Saussure was holding historical linguistics in low esteem.[9] However, a close reading of Saussure's *Mémoire sur le système primitif des voyelles* makes these points very clear, indeed offering us in hindsight a proof for the desirability of a sound combination of theory and empirical evidence, of deduction and induction, in historical linguistic research. That his *Mémoire* has still today not yet lost its original lustre may be taken from recent evaluations of this work (e.g., Vincenzi 1978; Watkins 1978), and a recent announcement (Gmür 1980: 4) of a comprehensive dissertation devoted to this pioneering work.

Notes

[1] Saussure must have drawn some belated satisfaction from Brugmann's observation (1966 [1909]: 560) according to which Osthoff's monograph (1881) constituted in effect a step back ("Rückschritt") vis-à-vis the *Mémoire*.

[2] Curtius was not referring to the *Mémoire*, whose rigor might well have exceeded his tastes and comprehension, but to an article of 1884, "Une loi rythmique de la langue grecque", of which Curtius had taken notice, one year before his death, which had probably been precipitated by the 'war of monographs' between him and his former students concerning the sound laws and the analogy principle during 1885. – The quotation is taken from Redard (1978: 36).

[3] Watkins (1978: 67), who quotes the second half of the sentence, argues that the phrase 'structure considérée en elle-même' italiziced in the citation constitutes "la véritable source" of a part of the famous statement at the end of the *Cours*, for which Engler was at a loss to find a textual basis in the critical edition (p. 515): *la linguistique a pour unique et véritable objet la langue envisagée en elle-même et pour elle-même* (*Cours*, p. 317). This is undoubtedly an overinterpretation, but it merits further investigation in the light of the entire context of Saussure's argument, both in the *Mémoire* and elsewhere in his writings, published as well as unpublished.

[4] Source: Lehmann (1967: 233). I have ventured to introduce a couple of *Verschlimmbesserungen* into the translation on the basis of my understanding of the French original (Saussure 1879: 51–52), where, among other things, certain phrases are in italics, but which Lehmann presented in regular type. (The emphasis in the original appears to me to underscore the forcefulness of Saussure's manner of argumentation.)

[5] When Saussure speaks of "*i* et *e* passent de l'état *symphthongue* à l'état *autophthonge*" (1879: 8) – note also the use of "état" in the present context! – it seems to me that he is using a terminology (meaning "functioning as a consonant" and "functioning as a vowel", correspondingly) created by the French phonetician A. Dufriche Desgenettes (cf. Koerner 1976: 227–228).

[6] Interestingly enough, the great English phonetician Henry Sweet (1845–1912), in his review of the *Mémoire*, calls the sonant coefficients A and $\underset{\circ}{O}$ 'consonants' or 'consonantal elements' (Sweet 1880: 160). One may wonder whether it would be correct to say with Jonsson (1978: 2, n.1) that "we have to do [here] with an anachronism or a misinterpretation under the influence of the later consonantal theory", without indicating what he meant by 'later'. However, Sweet (1880: 158) refers to Möller's (1879) review of Kluge (1879), where in fact the consonantal hypothesis was first enunciated (see 1.2. below for details).

[7] Mayrhofer (1981: 28–29, note 81) adduces evidence for his suggestion that the term 'schwa' was first introduced by August Fick; since he refers only indirectly to the possibly first appearance of the term in print, reference is made here to Fick's paper (1879).

[8] Pedersen had been under the influence of Möller's views from very early on in his scholarly activity (from 1893 onwards, if not earlier); cf. the bibliography of his major writings in H. Pedersen (1983: xxiii–xxx). For details on Pedersen's contribution to the laryngeal theory, cf. Szemerényi (1973: 11–12 and 15).

[9] Thus, ever since the availability of Rudolf Engler's critical edition of the *Cours* (1967–1974), it should have become clear to everybody that the various statements in the text as compiled by Charles Bally and Albert Sechehaye concerning the clash between synchrony and diachrony for instance have little foundation in Saussure's lectures as recorded by his students.

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Archaic ablaut patterns in the Vedic verb

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1. The first singular active form of the Vedic sigmatic aorist injunctive does not take *vṛddhi.* This is a remarkable archaism which has not been sufficiently appreciated.

2. In his article on the "proterodynamic" root present Insler calls attention to the fact that "the system of proterodynamic present inflection reflected in Vedic forms is nearly identical to the oldest system of Vedic sigmatic aorist inflection" (1972: 56). "It is only when we compare the act. indic.-inj. of proterodynamic root presents that the complete parallelism breaks down" (Insler 1972: 57). The active forms of the sigmatic aorist have lengthened grade vocalism throughout the whole paradigm and do not show the expected alternation between lengthened grade in the singular and full grade in the plural which is found in *tāṣṭi, tákṣati.* We must therefore ask the question: "which paradigm seems to continue the original ablaut relationship?" (Insler 1972: 58).

3. The obvious explanation is that the active paradigm of the sigmatic aorist "has participated in the same sort of leveling of vocalism observed in act. root aorists of the type *ákar, ákarma, ákarta*" (Insler 1972: 58). Insler rejects this view because the lengthened grade vocalism was extended to the third plural form of the sigmatic aorist, whereas the corresponding form of the root aorist maintains the original zero grade, e.g. *ákran.* The argument does not hold because the ending of the root aorist was *-an* < **-ent*, whereas the sigmatic form ended in **-sat* < **-sn̥t.* The ending **-at* was replaced with *-ur*, as it was in the injunctive *takṣur* and in the reduplicated imperfect. The retention of the ablaut contrast in the paradigm of *tāṣṭi* and the extension of the lengthened grade to the third plural form of the sigmatic aorist fit "the general tendencies of the Vedic verb system to characterize act. athematic present inflection by ablaut differences, but to mark act. athematic aorist inflection by the predominant absence of any alternating vocalism" (Insler 1972: 61).

4. Lengthened grade vocalism was generalized in the active paradigm of the sigmatic aorist indicative, but not in the injunctive, which betrays the original distribution of the ablaut grades. It is noteworthy that the original distribution was already indicated by Wackernagel in his Old Indic grammar (1896: 68): the lengthened grade spread from the monosyllabic second and third singular forms to the rest of the paradigm. The archaic character of this distribution is supported by the Balto-Slavic evidence (cf. Kortlandt 1975, App. E, and 1984, sections 1.3 and 1.4). It is also clear from the Vedic material.

5. The first singular indicative has lengthened grade in RV. *ajaiṣam, aprākṣam, abhārṣam, ayāṃsam, aspārṣam, ahārṣam, akāniṣam, akāriṣam, acāriṣam, asāniṣam*, and ambiguous

vocalism in *ayāsam*. It has full grade in *akramiṣam* and in the analogic forms *akramīm* and *aśamsiṣam*. The first person singular injunctive has full grade in VS.TS.TB. *jeṣam*, TS.KS. TB.JB. *yoṣam*, and RV. *stoṣam, vadhīm*, and lengthened grade in the analogic form *rāviṣam* (*ru-* 'break').

6. Following Hoffmann, Narten interprets *jeṣam* and first person plural RV. *jeṣma* as precative forms (1964: 120). The reason for this interpretation is evidently the absence of lengthened grade (cf. Hoffmann 1967a: 254). The functional evidence for the interpretation as precative (Hoffmann 1967b: 32f) or subjunctive (Insler 1975: 15[26]) is very weak, while the formal objections against it are prohibitive. It is therefore preferable to retain the traditional view that these forms are what they look like: full grade injunctive forms, which were interchangeable with the corresponding subjunctive in certain contexts and which could be interpreted as precative when the latter category became common.

7. Narten assumes that the injunctive forms *yoṣam* and *stoṣam* took their vocalism from the subjunctive (1964: 213, 277). The model for this analogic development is lacking, however, because the subjunctive ending was *-āni*, not *-am*. Hoffmann attributes the alleged substitution of the injunctive ending *-am* for the earlier subjunctive ending *-ā* to the influence of the second singular imperative: "Das Bestreben, den Konjunktivausgang *-ā* von dem durch Auslautsdehnung gleichlautend gewordenen Imperativausgang zu sondern, hat das Ausweichen zu *-am*, wodurch die 1. Person deutlich gekennzeichnet wurde, gefördert" (1967a: 248). I find such influence highly improbable. The use of the first person singular injunctive for the subjunctive must be explained from the meaning of the forms. Note that standard British English offers an exact parallel in the use of 'I shall' where other persons 'will'.[1]

8. The indicative has lengthened grade in RV. third dual *asvārṣṭām*, first plural *ajaiṣma, abhaiṣma, atāriṣma*, second plural *achānta*, third plural *achāntsur, abhaiṣur, atāriṣur, apāviṣur, amādiṣur, arāniṣur, arāviṣur, avādiṣur, asāviṣur*, and ambiguous vocalism in *ayāsur, arājiṣur*, and *āviṣur*. It has full grade in third dual *amanthiṣṭām*, first plural *agrabhīṣma*, third plural *ataṣṭaṣiṣur, adhanviṣur, anartiṣur, amandiṣur*, all of which have a root in a double consonant (cf. *gṛbhītá-* < **gṛbhH-itá-*). It has zero grade in *amatsur, anindiṣur*, and *ākṣiṣur* (*naś-* 'attain').

9. The injunctive has full grade in Rgveda second dual *aviṣṭam, kramiṣṭam, gamiṣṭam, caniṣṭam, cayiṣṭam, mardhiṣṭam, yodhiṣṭam, vadhiṣṭam, śnathiṣṭam*, third dual *aviṣṭām*, first plural *jeṣma, śramiṣma*, second plural *aviṣṭa(na), grabhīṣṭa, raniṣṭana, vadhiṣṭa(na), śnathiṣṭana*, zero grade in *himsiṣṭa*, and ambiguous vocalism in third plural *dhāsur, hāsur*. It has lengthened grade in second dual *yauṣṭam* (ĀpMB. *yoṣṭam*), *tāriṣṭam*, second plural *naiṣṭa* (ĀpŚS. *yoṣṭa*), third plural *yausur, jārisur*, and in the analogic form second dual *yāviṣṭam* (*yu-* 'unite'). Note that the difference between first plural *śramiṣma* and *atāriṣma* parallels the one between *jeṣma* and *ajaiṣma*.

10. One may wonder if the ablaut difference between the indicative and the injunctive is also found in the asigmatic aorist. It has long been noticed that the third plural middle indicative forms *ákrata* and *árata* correspond with the injunctive forms *kranta* and *ranta* (Meillet 1920: 203, 205). The archaic character of this distribution is supported by the

isolated third plural injunctive forms *naśan* and *naśanta*, which correspond with indicative *ākṣiṣur* (for *āśur* replacing **āśat*) and *āśata*. Hoffmann's conjecture that the initial *n-* of the injunctive is of secondary origin (1957: 124f) does not explain why it is limited to the third person plural forms, cf. third singular middle *aṣṭa*. As in the case of the sigmatic aorist, it is probable that the vocalic alternation was eliminated in the indicative paradigm. This must have occurred at a much earlier stage, however, because it affected the form which was to yield *ásthur*. The full grade injunctive ending *-anta* survived in the paradigm of the subjunctive, which shared the thematic vowel. There is a trace of the original distribution in Homer *τάνυται, τανύοντο*.

11. As I indicated above (section 3), the third plural ending *-ur* replaced earlier **-at* < **-n̥t*, not *-an* < **-ent*. Since the optative ends in *-yur*, the original form must have had zero grade both in the suffix and in the ending. This suggests that it had full grade in the root.

12. Hoffmann has argued that the root aorist optative had fixed stress on the root (1968). His proposal offers a straightforward explanation for third plural Latin *velint*, Gothic *wileina*, and OCS. *velętъ*, but not for the remarkable alternation which the latter language shows between second plural *xoštete, dovьljete* and third plural *xotętъ, dovьlętъ*. It appears that the third person plural form differed from the other persons in the original paradigm. This enables us to remove the unlikely assumption that the root aorist differed from the root present in the accentuation of the optative.

13. Insler connects the type *dheyām* with the type *gaméyam*, the two being in complementary distribution (1975: 15). His explanation falters on two points. First, it requires the previous existence of both **dheyam* and **dhāyām*, of which the attested form represents a blending. It is highly improbable that neither of the earlier forms would have survived because both were supported by other paradigms, while the alleged blending created a new type. Second, the motivation for the spread of the new vocalism to the third person forms is very weak. The long chain of analogic changes which Insler's theory requires is too complicated to be credible.

14. Thus, I arrive at the following reconstruction of the Proto-Indo-European active root optative:

1st sg.	*dhH$_1$ieH$_1$m*	pl.	*dhH$_1$iH$_1$me*
2nd	*dhH$_1$ieH$_1$s*		*dhH$_1$iH$_1$te*
3rd	*dhH$_1$ieH$_1$t*		*dheH$_1$iH$_1$nt*

After Sievers' law and the loss of tautosyllabic laryngeals this paradigm turned into the following:

1st sg.	*dhiyām*	pl.	*dhīma*
2nd	*dhiyās*		*dhīta*
3rd	*dhiyāt*		*dha^{2}i^{2}at*

The generalization of **dha^{2}-* and the substitution of *-ur* for **-at* yielded first singular *dheyām*, third plural *dheyur*.[2]

15. The isolated first person plural middle optative form *naśīmahi* (three times) next to *aśīmahi* (five times) suggests that this paradigm also contained a form with full grade in the root. Since the initial *n-* is lacking elsewhere in the middle optative and indicative paradigms, it was probably taken from the unattested third person plural optative form.

16. The accentual mobility in the paradigm of the optative is reminiscent of the one in the reduplicated present, where third plural *bíbhrati* and *dádhati* have both initial stress and zero grade in the root and in the ending. Thus, I reconstruct PIE **dhedhH$_1$nti* 'they put'. It follows that the third person plural form does not have the same origin as the other forms of the paradigm.

17. The reduplicating syllable *da-* of *dádhāmi* replaces earlier *di-*, which is preserved in τίθημι and in the desiderative present *dídhiṣāmi*. It is difficult to agree with Leumann's view that *da-* was taken from the perfect (1952: 27) because the motivation for such an analogic development was very weak. More probably, the paradigm of the present contained a form with *da-* from the very outset. This must have been the third person plural form. In my view, PIE **dhi-* was simply the pretonic (zero grade) variant of **dhe-* before a double consonant, cf. πίτνημι, πίσυρες, Czech *čtvrtý* < **čьtvrtyj* 'fourth', OCS *šьlъ* < **šьdlъ* 'went'.

18. The third person plural forms *yánti, kranta, dheyur*, and *dádhati* have in common that the initial syllable contains a full grade vowel. They have the same vocalism as the participles *yánt-, kránt-, dádhat-*. It is therefore probable that the form in *-nti* represents the original nominative plural form of the participle. The plural ending *-i* is also found in the Proto-Indo-European pronominal inflection: nom. **to-i*, gen. **to-i-s-om*, dat. **to-i-mus*, abl. **to-i-os*, inst. **to-i-bhi*, loc. **to-i-su*. It follows from this point of view that the secondary ending **-nt* was created on the analogy of the singular forms, where the primary *-i* had a different origin.[3]

Notes

[1] During my stay in Dublin, Dr. Patrick Sims-Williams told me that when an Irish friend asked him in front of an open door: "Will I go first?", the only reasonable answer to him would be: "I don't know". Compare in this connection RV. VII 86.2 *kadā́ nv àntár váruṇe bhuvāni . . . kadā́ mṛḷīkā́ṃ sumánā abhí khyam* 'When will I be inside Varuna? When shall I, cheerful, perceive his mercy?' Also X 27.1 *ásat sú me jaritaḥ sā́bhivegó, yā́t sunvaté yájamānāya śíkṣam* 'That will be my excitement, singer, that I shall be helpful to the pressing sacrifier'. In X 28.5 *kathā́ ta etā́d ahám ā́ ciketam* 'How shall I understand this (word) of yours?' the substitution of the subjunctive for the injunctive would yield a quite different shade of meaning: it would shift the responsibility from the singer to Indra.

[2] Forms of the type *dheyām* are always trisyllabic in the Ṛgveda. This fits the explanation advanced here.

[3] In my view, the plural ending *-i* is of Indo-Uralic origin. It can be identified with the Fennic and Northern Samoyed oblique plural suffix *-i-*, e.g. Finnish *talo* 'house', pl. *talot, taloi-*. It is also found as a plural object marker in the Northern Samoyed objective conjugation, e.g. Yurak *mada-i-n* 'I (did) cut (more than two things)', cf. Finnish *pala-n* 'I burn (intr.)', Lappish *puolám* < **palak-mi*.

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Theoretical views affecting successive reconstructions of the phonological system of Proto-Indo-European

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0. Introduction

The phonological system proposed for a language, even for a reconstructed language, may seem to rest on the securest evidence available for that language, on evidence far more secure than that available for positing its syntactic system or its morphology. The changing views on the phonological system of Proto-Indo-European raise doubts concerning such an assumption. Within the period of scarcely more than one century, various phonological systems have been proposed for Proto-Indo-European. All have been based on essentially the same data. Only the Anatolian languages provided significant additional data after their discovery in the first decade of this century, data especially crucial to those who place little credence in internal reconstruction. While the data are largely unchanged, the successive systems proposed illuminate the changing theoretical views of linguists during the past hundred years. Since historical theory in its development has been of special interest to the scholar honored by this volume, I take this opportunity to review selected innovations in that theory with reference to their impact on the proposed phonological system of Proto-Indo-European.

Examination of the posited systems in turn provides perspectives on those in handbooks of the past, perspectives which may also be useful for evaluating phonological constructs proposed today and in the future. In presenting the systems proposed by selected Indo-Europeanists, I am primarily concerned with the theoretical views on which they were based, rather than with an optimum reconstruction of the phonology of Proto-Indo-European.

1. Schleicher and his system

The first system presented here is that of August Schleicher (1871: 10). Earlier linguists did not propose a separate Indo-European system, assuming instead that that of Sanskrit applied too for the parent language. A glance at Schleicher's system indicates that even it is heavily influenced by the Sanskrit system. Schleicher does not posit *e* or *o*. He does not include *l*. There are no labiovelars.

Table 1

Schleicher's phonological system

	Consonanten							Vocale		
	momentane laute			dauerlaute						
	nicht aspirierte		aspiratae	spiranten		nasale	r-laute			
	stumm	tön.	tönend.	stumm	tönend.	tön.	tön.			
gutt.	*k*	*g*	*gh*					*a*	*aa*	*ā̆a*
pal.					*j*			*i*	*ai*	*ā̆i*
lingu.							*r*		*au*	*ā̆u*
dent.	*t*	*d*	*dh*	*s*		*n*				
lab.	*p*	*b*	*bh*		*v*	*m*	*u*			

In keeping with our focus on the theories underlying posited systems, I limit myself to a few essentials rather than recalling other highly interesting remarks of Schleicher concerning his own views in comparison with those of his predecessors and colleagues.

First, Schleicher assumed an earlier system, which we might label pre-Indo-European. Such an assumption is admirable; it has been far too often neglected by subsequent Indo-Europeanists. For Schleicher, the system of Table 1 developed from an earlier one lacking voiced aspirates and diphthongs with *ā̆*. An even earlier system, the most original ("ursprünglichsten") lacked all diphthongs. His system of 1871 is that of the language 'shortly before the first division.'

Second, Schleicher clearly posited a symmetrical set of sounds. From it we may conclude that he aimed to propose a system, not simply a list of individual sounds.

His aim to posit a system apparently had further consequences, of which I cite two. One concerns the absence of laryngeals. In a note he states that the 'aleph or hamza of the Semites' was found before initial vowels. The omission of this element from his roster gives us insights into his theoretical views. First, he did not regard morphological structure in proposing his system. Further, the aleph was disregarded as an element not in direct contrast with other consonants and accordingly extra-systematic. – As an aside we may wonder whether his suggestion of a laryngeal in the system may not have had important consequences. Saussure, whose monograph is dated eight years later, surely knew Schleicher's *Compendium*. So must have Möller, who identified Saussure's (1879) coefficients as laryngeals in the year 1880.

A second consequence of Schleicher's systematization is his positing of *b*. Schleicher states that he knows no completely certain examples of this sound, adding that Grassmann made it highly probable that *b* was not present in the proto-language, at least not initially (1871: 160). Systematization apparently led Schleicher to include it nonetheless.

2. The neogrammarians

The following decade produced an explosion of contributions on the phonological system. The period abounded with new ideas and argumentation. Already in Schleicher's day two

rival camps disputed the importance of adhering strictly to sound laws. Among those in favor of doing so Schleicher lists G. Curtius in Leipzig, Corssen in Berlin and himself. (In spite of this classification, Curtius comes down to us as superseded father of the neogrammarians.) Among modifications of Schleicher's system introduced by adherents to sound laws are the vowels *e* and *o*; unshakeable evidence was provided from Sanskrit in the so-called law of palatals.

Insistence on sound laws led to concern with all residues, also termed exceptions to sound laws. Verner's explanation of the third set of exceptions to Grimm's rules, exceptions well-known for fifty years, led to the assumption of suprasegmentals, and to clarification of the Indo-European system of ablaut. In short order, Schleicher's system was completely superseded. The course of the modification and their bases are well-presented in Fritz Bechtel (1892). In this survey Bechtel includes publications to January 1890, less than 20 years from the publication date of Schleicher's third edition of the *Compendium.*

Examination of Brugmann's phonological system discloses changes that had been adopted in these few years. I provide the system as presented in Brugmann (1904).

Table 2

Brugmann's phonological system

1) Vokale. i ī, u ū, e ē, o ō, a ā, ə ('Schwa'). – i̯, u̯. – Fallende Diphthonge: ei oi ai əi, ēi ōi āi; eu ou au əu, ēu ōu āu.

Nasale. m (labial), n (dental), n̂ (palatal), ŋ (velar). Silbisch: m̥ m̥̄, n̥ n̥̄, n̥̂ n̥̂̄, ŋ̥ ŋ̥̄.

Liquidae. r, l. Sonantisch: r̥ r̥̄, l̥ l̥̄.

2) Verschlußlaute (Explosivae).

p	ph	b	bh	(labial, genauer bilabial),
t	th	d	dh	(dental, genauer vermutlich alveolar),
k̂	k̂h	ĝ	ĝh	(palatal),
q	qh	g	gh	(reinvelar),
q^{u}	$q^{u}h$	g^{u}	$g^{u}h$	(labiovelar).

3) Reibelaute (Spiranten). s und sh, z und zh; þ und þh, ð und ðh. Dazu vielleicht j.

Clearly the elements in the system have been vastly increased. Brugmann posits five vowels in long and short form, plus schwa. These are involved in 14 diphthongs, so that the vocalic system alone includes 25 entities. If we include among the vowels syllabic resonants, the total is 37. In addition there is a large number of stops, nine fricatives and eight consonantal resonants. I do not deal here with theoretical views on accent, but I should note that Brugmann also posited stress and pitch.

When we identify the theoretical bases underlying the massive expansion of the system, we note that Brugmann's phonological entities are basically phonetic. The separate entities representing vocalic and consonantal resonants provide a clear example. Brugmann knew of their alternation. He cites examples which Sievers had discussed in the article presenting what came to be known as Sievers's law. Nonetheless he regarded the alternates as distinct. Brugmann's basic phonological entity is the 'Sprachlaut', not the phoneme. While 'Sprachlaut' implies a class rather than a simple sound, the class is determined by phonetic criteria rather than by relationships in a system. For Brugmann, phonological classes are characterized then by their phonetic value not by their interrelationships.

In spite of this use of the 'Sprachlaut', Brugmann sought system in language. This assumption may be illustrated by the set of vocalic elements, including long vocalic resonants; one is posited for every characteristic articulatory position, however little evidence we have for it. Adherence to systematization is almost grotesque in the set of fricatives, which are given the pattern of the stops; Brugmann finds little evidence for aspirated *s* and *z* in his detailed treatment of the fricatives, yet he posits them. Scrutiny of his phonological treatment leads one to suggest that he might have done well to avoid the notion of system.

Hirt, Brugmann's successor in producing a comprehensive Indo-European grammar, followed similar theoretical procedures (1921–37; 1939: 116–165). He proposes even more vocalic elements than does Brugmann, including rising as well as falling diphthongs. The theoretical approach of these two important Indo-Europeanists is based primarily on consideration of phonetic entities which were parallel in their distribution.

3. The classical phonemic approach

Classical phonemics was developed in the twenties and thirties of this century, after Brugmann's death and after publication of the phonological volumes of Hirt's Indo-European grammar. The theoretical effects are already apparent in the great French Indo-Europeanist, Antoine Meillet, who died in the same year as Hermann Hirt, 1936.

In the 8th edition of Meillet's *Introduction*, published under supervision of Benveniste in 1937 with only slight revisions from the 7th edition of 1934, Meillet notes changes in linguistic theory following the publication of Saussure's *Cours* (1937: XIII–XIV). When, however, we compare Meillet's phonological system with that of Brugmann, we find only one major change. Rather than positing three classes of palato-velar obstruents, Meillet posits two: a labio-velar and a non-labialized palato-velar. This system accords with the two oppositions found throughout the Indo-European area: in some languages, between labio-velars and non-labio-velars, in the others, between palatals and velars. In keeping with Saussure's insistence on the importance of oppositions, only two sets are necessary, as presented by Meillet (1937: 93–94).

Most subsequent Indo-Europeanists interpret the data as did Meillet. The individualistic Polish linguist, Jerzy Kuryłowicz, also posited only two sets (1935: 1–26). But these are palatals and velars. For him the languages with labio-velars represent innovation.

While Meillet's revised system shows only slight modification by the phonemic principle, subsequent scholars applied the principle much more thoroughly. Possibly the most significant application is Franklin Edgerton's, to the resonants. An introductory article on the problem in 1934, 'Sievers's law and Indo-European weak grade vocalism', was followed by an extensive essay in 1943, 'The Indo-European resonants'. In these articles Edgerton analyzed the Indo-European system rigorously in accordance with the principles of classical phonemics.

Following Sievers's conclusions based initially on Gothic, Edgerton concluded that consonantal and vocalic forms of the six resonants stand in complementary distribution, as reflected in Gothic data.

Table 3

Gothic data illustrating variation of Gmc *i:j:ij*

1st sg. pres. act.	nasja	stōja	sōkja	(Edgerton B)
2nd sg. pres. act.	nasjis	stōjis	(sōkeis < *iji	Edgerton C)
1st sg. pret. act.	nasida	stauida	sōkida	(Edgerton A)

Relying especially on Vedic data, he assumed for Proto-Indo-European three allophones for each resonant: A. vocalic, B. consonantal, C. vocalic + consonantal. These are precisely specified in a series of formulae like the following (1943: 108):

A tit /it ti/ (/ = word boundary)
B aya /ya ay/
C ăty a but ktiya ātiya / tiya

By this view twelve of Brugmann's and Meillet's phonological entities are reduced to six. The formulation had a powerful appeal. The Indo-European phonemic inventory seemed more realistic with its smaller numbers. The variants were governed by precise rules. Subsequently this description of variation in the Proto-Indo-European resonants is often referred to as the Sievers-Edgerton law.

Acceptance of the Sievers-Edgerton law entails further modifications of Brugmann's inventory. It eliminates the diphthongs. If *-y-* and *-w-* are variants in well-defined environments, their combination with preceding *e* and *o* cannot be viewed as distinct phonological entities.

Moreover, the vowel system is reduced. There is no basis for proposing the five short vowels of Latin for Proto-Indo-European. Only three may be assumed.

Further, the environments for Edgerton's set here labeled C might be explicated. If vowel length plus consonant is equivalent to consonant plus consonant, the environment might actually be two preceding consonants or their equivalent. Rather than long vowels as in Go *sōkeis*, combinations of short vowel plus a consonantal element might be assumed for the time when the law was in effect. That consonantal element might be equated with Saussure's coefficients, Möller's laryngeals.

These in the meantime had gained greatly in credibility by Kuryłowicz's publication of 1927 pointing out that Hittite texts included forms for which Saussure had posited a coefficient. Many of these forms had a consonant at the place of the coefficient, a consonant indicated by ḫ in our transcribed texts (e.g. Kuryłowicz 1935: 73–74). Orthographic evidence had been established for the consonantal elements which Saussure had proposed on the basis of the method of internal reconstruction.

The Sievers law phenomena in this way added credence to the laryngeal theory, at the same time providing evidence for different functioning of the resonants from the laryngeals. Four sub-sets may then be proposed for the Proto-Indo-European phonological system: Obstruents, Laryngeals, Resonants, Vowels.

Table 4

System taking Sievers's law and the laryngeal theory into consideration

1. Obstruents:	p	t	k	k^w		
	(b)	d	g	g^w		
	b^h	d^h	g^h	g^{wh}		
			s			
2. Laryngeals:					?	
					x	h
					γ	
3. Resonants:	m	n				
	w	r	l	y		
4. Vowels:	i:				u:	
	e:	e	ə	o:	o	
			a:	a		

Controversies concerning the laryngeal theory are of lesser concern to us here, though varying views are of interest in illustrating the effect of differing theoretical approaches. Some Indo-European phonologists continued to treat the laryngeals as abstract entities, e.g. Benveniste and Kuryłowicz, who used the symbols: $\underset{\circ}{\text{ə}}_1$ $\underset{\circ}{\text{ə}}_2$ $\underset{\circ}{\text{ə}}_3$ ($\underset{\circ}{\text{ə}}_4$). Others identified them phonetically, e.g. Sapir and Sturtevant. Some posited sub-systems of lesser or of greater numbers, ascending as high as 8. Determination of the number of laryngeals rests in part on reconstruction of several prehistoric stages (cf. Lehmann 1952: 112–114; Meid 1973: 204–219; Neu 1976: 239–254) or only one, in part on one's theoretical views concerning system in language. Sapir in his balanced way assumed laryngeals for which he found reflexes and also parallels in other languages (1938); in this way he introduced typological considerations, which subsequently became highly prominent in treatments of the early system. With arguments that are difficult to ignore, he proposed four laryngeals, as given in Table 4.

Phonologists were aware of problems with this system. The realization of the third row of obstruents was unclear. As long as the previous system including *ph th kh* k^wh was maintained, the symmetry provided cogent support for a balanced set of entities; one could even assume realization of the sets transliterated with *h* as voiceless and voiced aspirated stops. But when the *ph* series was eliminated, after acceptance of the laryngeal theory, a system including voiced aspirated stops in contrast with voiceless and voiced stops seemed highly unusual. Even earlier some linguists, among them Eduard Prokosch, held these entities to be voiceless fricatives, buttressing this assumption with reflexes like Latin *frater* with its *f* (1939: 57–59). No solution was however widely accepted.

A second problem was given less attention at this time, the lack of evidence for *b* noted already by Schleicher. In general, Indo-Europeanists pointed to this oddity but did little more about it.

A further concern arose from scrutiny of the Vedic texts for treatment of the resonants. The Vedas contain many examples of the functioning of Sievers's law, but there is also contradictory evidence. For Edgerton, the contradictory evidence is found in late texts, produced after a time when the law was in force. The aberrant patterns seemed to him parallel to umlaut introduced in many forms which lacked the phonological structure triggering

it, such as German *Generäle* 'generals' with umlaut in a word brought into German after the period of the umlaut changes and *Löcher* 'holes' with umlaut in a pattern that should have umlaut of *u* rather than *o*, and many more, restructured by the process traditionally referred to as analogy. Other scholars took a different view, regarding the Vedic situation as more accurate evidence than the theoretical assumptions of Sievers, Edgerton, and those who accept Sievers's law.

The conflict between the two views intensified, leading to revisions in the theoretical approach of linguists such as Kuryłowicz. By this approach which Kuryłowicz allowed in his later years, many phonological alternations in morphological sets were to be explained without assuming origin in a phonological change, but rather through morphological structuring alone. As a further example we may cite Brugmann's law, a proposal to account for *ā* rather than short *a* as the reflex in some Sanskrit forms for Proto-Indo-European *o*. The perfect provides examples illustrating the difficulty. This commonly has a short vowel in the first singular, as in *tatana* (also however *tatāna*) 'I stretched', but long vowel in the third singular, as in *tatāna* 'he stretched'. In its strict form Brugmann's law proposes change of short PIE *o* to Skt. *ā* before resonants in open syllables. Before the laryngeal theory was proposed, the contrast between the first and third singular stem vowels was unexplained. After the Hittite *h*-conjugation came to be known, with *-hi* in the first singular, *-i* in the third, it became clear that the first singular had *o* in a checked rather than in an open syllable, and accordingly was not subject to Brugmann's law.

Like the Sievers's law phenomena, however, the expected reflexes of Brugmann's law frequently are not found. Moreover, long *ā* for PIE *o* is attested in some forms before obstruents. By the classical theory of historical linguistics, the aberrant forms are accounted for by analogy; at one time in early Indic, short *o* in well-defined environments became long *ā*. By the contrasting view, there never was a phonological change; all the long *ā* were morphologically triggered.

The expanded role of morphology may seem based on the generative view of systematic as opposed to classical phonemics. The generativists however did not concern themselves to any extent with Proto-Indo-European phonology; it is difficult to cite any work by linguists practicing the generative transformational approach which contributed to our understanding of Proto-Indo-European segmental phonology. It is possible that linguists outside the central generative transformational sphere, such as Kuryłowicz, took over some notions of systematic phonemics. In any event, the confrontation between the two approaches may have led to acceptance of greater flexibility in applying classical historical theory.

The flexible view may be summarized as follows:

(1) Phonological contrasts may be introduced into a linguistic system. The contrast may arise variously: a) by a sound change in the classical sense, as when American English medial /t/ in specific environments falls together with /d/ for many speakers, e.g. *bitter : bidder, beatle : beadle*; b) by introduction of a contrast through borrowing, whether from another dialect or another language, e.g. English /ž/ as in *beige*.

(2) The change may be established and extended to further speakers, or it may be eliminated.

(3) If it is established and extended, it may furnish the basis of expressing specific meanings, whether grammatical or lexical. Such extension for morphological purposes has not

happened for English /d/ as opposed to /t/. We could however develop a further means of distinguishing between present and past from the phonological contrast *t* : *dəd* in such pairs as *fit* : *fitted, seat* : *seated.* Such a contrast would be useful in overcoming the ambiguity in current English verbs like *beat*; it may well be that such verbs are too few to warrant a distinction. If however speakers perceive a need for a specific morphological contrast, they may adopt one from such phonological contrasts in a language.

Umlaut in the late Germanic languages provides an example we have already drawn on. The source of the change is assimilation of stem vowels to high vowels in the remainder of the word, either later *i* or *u*. The umlaut change became phonemic through a further change – weakening and often loss of the triggering vowels; by that loss a means for distinguishing some plurals from singulars was also forfeited. Umlaut of stem vowels provided a means for a new plural marker. Its morphological application in English plurals like *men* : *man, geese* : *goose, mice* : *mouse* is relatively limited; but Standard German provides many nominal examples, and also examples in the verb, like that between the third singular present of *geben* : *gibt*, and the second plural *gebt.* A clear statement of this classical view was given by Weinreich, Labov and Herzog in 1968, especially the processes of embedding and extending the results of a change.

As a result of scrutiny of specific problems in accordance with the phonemic approach, the theoretical position of Indo-European phonologists increased in explicitness as well as in flexibility. After Schleicher the great advance came in careful phonetic analysis. The phonemic approach yielded credible systems from the large numbers of phonetic elements posited by Brugmann, Hirt and their contemporaries. It was however applied almost exclusively to data in the Indo-European language family. Subsequently phonological theory has been reviewed in accordance with the findings of typology.

4. Typological contributions

By a typological approach one compares entities for specific properties, attempting in this way better to understand those entities within a given system and given range of functions. One could, for example, attempt a typology of vehicles, classifying them for physical or surface characteristics. A deeper classification would take function into consideration. Additional scrutiny might involve attention to a social group, which like the early Indo-European speakers made use of wagons and chariots in contrast with many of their contemporaries. Typology then does not confine itself to external form.

Typology has been pursued since the early days of modern linguistics, paralleling the predominant concerns of the discipline though never as prominent as historical or purely descriptive study. Beside Grimm we can place von Humboldt, who was followed by the eminent scholars Steinthal, Finck, W. Schmidt and Sapir. However brilliant, the typologists occupy a secondary position beside linguists like Grimm, Bopp, Schleicher, the neogrammarians and their students, such as Leonard Bloomfield and current scholars who disregard the approach. Roman Jakobson has stated for example that the typologists were "distrusted by the neogrammarians" (1962: 524).

Jakobson made this statement at the International Congress of Linguists at Oslo in 1957. Possibly because of the forum provided there his remarks influenced subsequent work on

Indo-European phonology, while Prokosch and others with similar views on the Proto-Indo-European obstruent system were largely disregarded. One of Jakobson's statements is especially noted: "To my knowledge, no language adds to the pair /t/ – /d/ a voiced aspirate /d^h/ without having its voiceless counterpart /t^h/, while /t/, /d/, and /t^h/ frequently occur without the comparatively rare /d^h/ . . . ; therefore theories operating with the three phonemes /t/ – /d/ – /d^h/ in Proto-Indo-European must reconsider the question of their phonemic essence" (1962: 528).

It was not this feature, however, that aroused new consideration of the Indo-European obstruent system, but rather the absence of /b/, which Schleicher had already noted. In the meantime typologists had found such a hole in the set of labial obstruents when the series involved was glottalized. Yet the absent glottalized labial was not voiced, but rather voiceless. This observation led to the glottalic theory for pre-Indo-European.

Much like the law of palatals in the 1870s, the glottalic theory was proposed simultaneously by different scholars, Paul Hopper in this country and Tamas Gamkrelidze with Vyacheslav Ivanov in the USSR. Their initial articles were published in 1973, and have been followed up by further publications. The glottalic theory assumes that *b* was missing in Proto-Indo-European because the earlier system included glottalic voiceless stops rather than a series of voiced stops. (To maintain clarity of presentation, the old labels of Grimm have been taken over for the three kinds of obstruents, as in a recent paper by Hopper (1982).) The sub-sets may be illustrated by the dentals.

	Tenues	Mediae	Mediae Aspiratae
Pre-Indo-European	t	t'	d
Proto-Indo-European	t, th	d	dh
Taihun (Proto-Germanic)	þ	t	d
Decem (Sanskrit)	t, th	d	dh

In this way the glottalic theory has suggested a solution for the ancient problem of missing *b*.

The theoretical basis for the glottalic theory differs markedly from those on which the modifications were proposed in the neogrammarian periods and the period of structuralism. Each of these theoretical approaches made use of the primary methods applies in historical linguistics, methods which Hoenigswald has acutely scrutinized and elaborated (1960, 1973). Both the comparative method and the method of internal construction require data in the languages to which they are applied. Conjectures regarding the obstruent system, on the other hand, are derived in the absence of such data; reconstruction of the "mediae aspiratae" to be sure is suggested on the basis of reflexes in some dialects though as past consideration has shown the data leave broad rein to the imagination; data for reconstructing glottalics are absent, permitting inferences based only on generalizations drawn from scrutiny of phonological systems in language.

Not surprisingly the glottalic theory has encountered considerable scepticism, as may be concluded from the successive publications of the principal proponents of the theory. Fortune may bring support like that of Hittite for Saussure's coefficients. The chances are greater for evidence from internal features of the known dialects which may provide such support through clues interpreted with improved understanding of language based on increasing scrutiny of phonological characteristics in typological study. Even then strong

support will be needed to offset the scepticism resulting from the greater complexity of historical developments required by the glottalic theory than from a system of Proto-Indo-European obstruents like that proposed by Prokosch. Formerly Germanic and Armenian were assumed to have undergone far-reaching obstruent shifts, as formulated in Grimm's Law and the rules for the Armenian shift. Now such shifts must be assumed for all subgroups of the family. The older view accordingly represented far greater economy.

Further, proponents of the glottalic theory can provide no support other than that from typology. They are accordingly in the position of Saussure in 1878, whose coefficients were proposed purely on a theoretical basis. But for the glottalic proponents there has not been a Möller to identify their newly proposed consonants with possible cognates in another set of languages like the Semitic.

Summing up, we note three major periods and kinds of innovative theory.

(1) After Schleicher's proposal of a phonological system for Proto-Indo-European, great elaboration was provided by the increased understanding in phonetics. The phonetic handbooks of Sievers, Jespersen and Sweet have still scarely been surpassed for articulatory phonetics. Illustrations of their mastery are available in contemporary articles on Indo-European phenomena.

(2) Development of phonemic theory contributed recognition of interrelationships between the many entities posited by the phonetic approach. It also led to a compact system composed of sub-systems. This system was examined for morphophonemic relationships. As sketched above, the morphophonemic relationships treated in Sievers's law helped support the laryngeal theory. This theory gained further credence from its elucidation of other phonological cruxes, such as the Brugmann's law phenomena, and from its clarification of relationships between morphological sets, such as the fifth, seventh and ninth verb classes of Sanskrit. Yet the massive evidence from morphology led some linguists to locate the basis of phonological cruxes in the morphological rather than the phonological sphere (Kuryłowicz 1977: 6–52, and elsewhere; see also Seebold 1972). The clash in theoretical positions has by no means been resolved, though both sides proposed a relatively similar phonemic system for Proto-Indo-European.

(3) To this phonemic system with its detailed phonetic analysis the results of typological theory are being applied, especially to earlier stages of the system. Assuming continued scrutiny comparable to that in former innovative concern, we may expect continued attention to typology; this may yield improved understanding of phonological cruxes, such as those in the suprasegmental system, and as a result gain greater credence for the glottalic theory.

These brief considerations of theoretical views may illustrate how differences in theoretical position affect the conclusions based on data, even though the data themselves are scarely modified. Such effects may be most noticeable in dealing with reconstructed languages though they are not absent from treatments of languages or other social systems directly available to us.

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L'articolo indeterminativo
(note per la storia della grammatica italiana)

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A prima vista, l'esistenza e il valore dell'articolo indeterminativo (*un*), a fianco di quello determinativo (*il*), possono apparire ovvi e non problematici, e l'impressione sembra confermata dalle grammatiche scolastiche, e da quelle correnti per lo studio delle lingue straniere: *il* corrisponde al francese *le*, all'inglese *the*, al tedesco *der*, ecc., e *un* al francese *un*, all'inglese *a*, al tedesco *ein*, ecc.

Ma basta un momento di riflessione per accorgersi che la faccenda non è così semplice. I due articoli non sembrano opporsi l'uno all'altro significativamente, costituendo una naturale classe paradigmatica. Nella maggior parte dei casi non si ha scelta, ma è la struttura dell'informazione nel discorso che impone l'uso dell'indeterminativo per l'introduzione di un elemento nuovo, e del determinativo per uno già nominato (come indica l'univocità delle scelte nelle traduzioni da lingue senza articoli a lingue con articoli: cfr. Lepschy (1980)). Quanto alle implicazioni logiche, basta scorrere un manuale (per es. Reichenbach (1947)) per vedere che i problemi posti dall'indeterminativo (nei suoi usi, specifico, non specifico, e generico, e per l'inclusione e l'appartenenza a una classe) sono molto diversi da quelli delle descrizioni definite che Russell associa all'uso del determinativo (per una discussione in ambito generativistico cfr. Hawkins (1978); per una sintesi di linguistica generale cfr. Krámský (1972); e per l'italiano cfr. Renzi (1976b)).

L'indeterminativo presenta delle difficoltà specifiche. Un italiano che cominci a studiare l'inglese si troverà incerto se dire, in corrispondenza di *un etto, un chilo*, ecc., *an ounce, a pound*, oppure *one ounce, one pound*, ecc., e si renderà conto che la sua incertezza non riguarda soltanto l'uso inglese (se entrambe le forme, con *a* e con *one*, siano possibili, e, se lo sono, come differiscano), ma anche il valore della forma italiana: si tratta dell'articolo indeterminativo, o del numerale *uno*? E, di fronte alla difficoltà di dare una risposta, ci si può chiedere se la distinzione sia necessaria, o, in altri termini, se l'indeterminativo di fatto esista, o se non si possa farne a meno, e interpretarlo come un numerale. Di fronte a *vorrei una mela*, ci si può chiedere a che cosa si opponga questo *una*: il senso (a) di numerale si oppone a *due, tre*, ecc.; come indeterminativo si oppone forse a *questa, quella*, o altri dimostrativi, almeno nel suo senso (b) di 'una qualsiasi'; nel senso (c) di 'una certa' si opporrebbe invece al senso (b). D'altra parte, il tipo di opposizione è diverso: mentre riferendosi al senso (b) sarebbe possibile rispondere: 'ecco, ti darò questa', riferendosi al senso (a) sarebbe strano rispondere: 'ecco, te ne darò due'. Per il francese l'interessante grammatica di Martinet osserva:

"L'article *un* [. . .] est homonyme du cardinal *un* [. . .] Il arrive fréquemment qu'on ne sache auquel on a affaire, ou même que les deux valeurs soient combinées et présentes dans l'énoncé" (1979: 43).

Perfino in una lingua come l'inglese, in cui l'articolo indeterminativo (*a, an*) ha una forma diversa dal numerale (*one*), è stato proposto, nell'ambito della grammatica generativa, di vedere nel primo una realizzazione non accentata del secondo (Perlmutter (1970); e cfr. per l'italiano le obiezioni di De Boer (1972) e di Renzi (1976a)). Come per molte altre ipotesi generativistiche è interessante notare che qui abbiamo un'interpretazione che da un lato ricapitola nella derivazione sincronica uno sviluppo diacronico, e dall'altro riecheggia idee di grammatici del passato. Questi infatti hanno esitato a lungo, prima di appaiare l'articolo indeterminativo a quello determinativo, ed è appunto sulle loro discussioni che intendo soffermarmi in questo saggio.

Come è noto, presso i grammatici greci *arthron* poteva comprendere, oltre all'articolo, anche pronomi di vario tipo (insieme ad altre parti del discorso), e i grammatici latini, trasferendo nozioni e termini greci al latino, che per di più non aveva l'articolo, non introducono chiarezza in questo settore. Le prime descrizioni grammaticali dei volgari europei moderni, impostate sulle grammatiche latine, e applicate a lingue che, a differenza del latino, usano gli articoli, rivelano considerevoli oscillazioni. Per il francese la storia dell'articolo nella tradizione grammaticale è stata tracciata da Yvon (1955–1956); e per l'inglese la classificazione dell'articolo fra le parti del discorso viene tabulata nella monografia di Ian Michael (1970: 532). L'articolo indeterminativo viene a volte associato a quello determinativo, ma stenta a farsi strada. Nella tradizione inglese i termini 'finite' e 'infinite' si trovano in Ben Jonson (1640), e 'definite' e 'indefinite' in J. Howell (1662). In quella francese *un* viene considerato un articolo 'improprio', e i termini 'infini', 'indéfini' vengono per lo più riferiti alle forme invariabili, cioè alle preposizioni. Bisogna arrivare alla *Grammaire générale* di Port-Royal per trovare una descrizione che a noi appare singolarmente moderna, in cui si oppone l'articolo 'défini' (*le, la*) a quello 'indéfini' (*un, une*). Ma le principali grammatiche successive, anche per influenza di Du Marsais e dell'Enciclopedia, accettano solo l'articolo determinativo, come, tanto per dare un esempio, nella *Grammaire des grammaires* di P. Girault-Duvivier (1812), e, fra la grammatiche pedagogiche, un quella diffusissima di F.J. Noël e Ch. P. Chapsal (1823, ristampata fino al 1888), che non parla affatto dell'indeterminativo.

Per la tradizione italiana, Kukenheim (1932: 126) ha osservato che, nonostante l'acume rivelato dai nostri grammatici, si è tardato molto a rendersi conto della funzione particolare dell'articolo indeterminativo. Va notato peraltro, insieme al ritardo, un anticipo, se è vero che la prima discussione importante sull'uso di *un*, è quella di Salviati (1586).

Senza pretendere di fare un esame sistematico, ho dato una rapida scorsa a varie grammatiche che mi erano facilmente accessibili (per un regesto, generalmente attento anche alla questione dell'articolo, si veda la vecchia ma insostituita *Storia* del Trabalza (1908)). Fin dall'inizio, si trova, nella presentazione dell'articolo, una serie di preoccupazioni diverse:

(a) l'articolo come particella che serve ad illustrare il genere, il numero e il caso della parola, un po' come in Prisciano la declinazione era accompagnata dalle forme di *hic* (che del resto era chiamato 'articulus', o 'pronomen articulare'): *hoc mare, huius maris, huic mari, ab hoc mari*; e così *el cielo, del cielo, al cielo, dal cielo.* Questo porta ad associare

l'articolo alla preposizione, e ad esaminare l'uso delle preposizioni articolate. Naturalmente a noi questa impostazione sembra trascurare la disparità fra il latino, in cui le desinenze dei nomi indicavano simultaneamente genere, numero e caso, e l'italiano in cui esse indicano genere e numero, ma non caso.

(b) Vengono precisate le forme diverse che l'articolo (con le sue variazioni di genere e di numero) può avere (per es., *il* o *el*), anche a seconda delle parole a cui si accompagna (per es., *il* o *lo*);

(c) vengono illustrate le regole per la presenza o l'assenza dell'articolo, spesso con grande penetrazione, e considerando anche complesse questioni sintattiche (si pensi ai casi come *le immagini della cera, una immagine di cera* esaminati dal Bembo);

(d) oltre che alla preposizione, l'articolo viene spesso associato al pronome, in particolare per i casi, che a noi appaiono di omofonia, di *il, lo, la*, ecc. come complementi oggetti, che vengono di solito interpretati come esempi di articoli usati in funzione di pronome;

(e) a volte vengono presentate norme che a noi non sono immediatamente ovvie, e che meriterebbero ancora di essere chiarite (per es., le osservazioni del Fortunio (1516: XI^{r}) sulle forme che vanno con gli aggettivi piuttosto che coi sostantivi; o quelle del Bembo (1525: 199 dell'ed. Dionisotti) sulla *e* che si introdurrebbe al posto della vocale iniziale di parola e di quella finale dell'articolo).

Ma si tratta, in ogni caso, dell'articolo determinativo. Non ho trovato menzione di quello che noi chiamiamo indeterminativo, non solo nelle prime grammatiche, ma anche in molte delle trattazioni più mature: per es., nell'opuscolo di L.B. Alberti (della metà del Quattrocento), nelle *Regole* del Fortunio (1516), nelle *Prose* del Bembo (1525), nelle *Tre fontane* del Liburnio (1526), che elencano gli articoli fra i pronomi, nella *Grammatichetta* del Trissino (1529), nella *Grammatica volgare* dell'Acharisio (1537), in quella di Tizzone Gaetano (1539), nelle *Regole* di Iacomo Gabriele (1545), nella *Grammaire italienne* di J.P. de Mesmes (1548), nei *Fondamenti* di R. Corso (1549), nelle *Osservationi* del Dolce (1550), in *De la lingua* di P.F. Giambullari (1551), nella *Giunta* del Castelvetro (1563), nell'*Italian Grammar* che H. Granthan (1575) basava sui *Praecepta* (1567) di Scipione Lentulo.

Ma anche dopo il Salviati continua a colpirci l'assenza di una trattazione adeguata di *un*, anche dove più ci si aspetterebbe di trovarla, come nella *Nouvelle méthode* italiana di Port-Royal (1660). Il Mambelli (1644) parla di *uno* come "pronome universale indeterminato" (cito da un'edizione del 1709, p. 392), e M. Kramer (cito dalla quindicesima edizione, del 1763, della sua *Vollkommene toscanisch- und romanische italiänische grammatica*) distingue "Articul oder Bestimmwoertlein" (*lo, la*) (p. 38), "Articulo indefinito" (come nella declinazione *Dio, di Dio, a Dio, Dio, da Dio*) (p. 48), e "Pronom. Indefinitum" (*uno*), che chiama anche "Articulus unitatis" (p. 377). Ma in generale l'indeterminativo viene ignorato, come per esempio nella *Grammar of the Italian Tongue* di John Henley (1719), nelle *Regole* di G. Gigli (1721), in *Della lingua nobile d'Italia* di N. Amenta (1724), nel *Traité* dell'Antonini (1726) dove si chiamano articoli indefiniti, secondo la tradizione francese, le preposizioni o l'assenza dell'articolo. Il Baretti nella sua grammatica italiana (1760) non parla dell'indefinito, e nella grammatica inglese osserva che l'articolo inglese *a* "ha un significato indefinito, e vuol dire, *one*, cioè *uno* o *una*, relativamente a *more, più*". Così non ne parla il *Maître italien* (1787) del Veneroni, né il *Dictionary of the Peculiarities of the Italian Language* (1820) del Santagnello, che per l'inglese *a, an* dà "uno, un, una, declined with the indefinite article *di, a, da*" (p. 39); né gli *Avvertimenti gramaticali* (1826)

di Sforza Pallavicino; né la *Grammaire italienne* del Vergani, che ho consultato in un'edizione londinese del 1836. E' vero che la *Grammatica* del Soave (1771, ma cito da un'edizione del 1815) distingue fra articoli "determinati" e "indeterminati": si usa *uno, una* "quando fra gli oggetti compresi sotto ad un nome universale, non si pensa che ad indicarne uno qualunque" (p. 8). Ma solo intorno alla metà del secolo si stabilizza la situazione che a noi appare normale: la grande *Grammatik der italiänischen Sprache* (1844) di L.G. Blanc dà i due articoli, "bestimmte" (o come meglio si dovrebbe dire "bestimmende"), e "unbestimmte", corrispondenti al tedesco *der* e *ein*, e osserva che "Letzterer, der unbestimmte, wird durch das Zahlwort *uno, a* vertreten: *l'uomo* (der Mensch), *un uomo* (ein Mensch)" (p. 168). Una nota avverte che il milanese ha una forma particolare per l'articolo (*on, ona*), distinta da quella del numerale (*vun, vuna*) (cfr. p. 643). Il Gherardini, nella *Appendice alle grammatiche italiane* (1843) nota che "Li Articoli sono dunque *determinativi* come *Il, Lo, La*, ec. Nondimeno si sogliono chiamare *Articoli* anche le voci *Uno* e *Una*, quantunque non valgano a determinar precisamente veruna cosa; onde son detti *Articoli indeterminativi*" (pp. 57–58). Così i due articoli si trovano in uno dei manuali più fortunati, la *Italian Conversation-Grammar* (1858) di Charles Marquard Sauer (dove si usano i nomi "articolo definito" e "indefinito", in corrispondenza a quelli inglesi "definite" e "indefinite"); nella *Grammatica italiana* di R. Fornaciari (1879) in cui si usano i termini "articolo determinato" e "indeterminato" (e si collegano gli articoli ai pronomi, dimostrativo e numerale rispettivamente); con i termini "determinativo" e "indeterminativo" nella *Grammatica* ("Approvata come libro di testo dal ministero della Pubblica Istruzione") di Cesare Mariani, uscita all'inizio degli anni Ottanta (cito da un'edizione del 1904); e con i termini "determinativo o determinato", "indeterminativo o indeterminato" nella *Grammatica* scolastica (1896) di Ettore Piazza (che cito da un'edizione del 1905).

Come ho detto sopra, la prima trattazione importante a me nota di quello che oggi chiamiamo l'articolo indeterminativo, è dovuta al Salviati, nel secondo volume degli *Avvertimenti* (1586). Qui l'indeterminativo non viene accoppiato al determinativo, ma viene messo in una categoria a parte, chiamata "accompagnanome", di cui si indicano le somiglianze e le differenze rispetto all'articolo, con un acume e una precisione da cui possiamo imparare anche oggi. Vale la pena di riportare il passo, che non richiede chiarimenti:

"Dal Nome, nell'opera del sentimento, tuttochè nome sia anch'ella, è forse da distinguere una certa parte del favellare, che *accompagnanome* in questi libri ci piace di nominarla: posciachè proprio titolo non l'è ancora, che noi sappiamo, stato dato nel volgar nostro: ne dal Latino, ò dal Greco, il possiamo torre in prestanza, che cotal parte non usarono in lor sermone, ne conoscerla, non che nomarla, non poteron per conseguente. Ed è questa, che noi diciamo, la voce *uno*, ò *una*, quando non come numerale, ma per una cotale accompagnatura si mette davanti a nome, che si ponga nel minor numero: che dirado non v'avendo l'articolo, senza essa lo troverrai: ma con esso articolo non vi puo mai aver luogo [. . .] alcuna forza porta nel sentimento, a quella dell'Articolo non intutto dissomigliante. perciocchè ristrigne anch'ella al Nome, come l'Articolo, e gli determina il suo valore: ma in cio sono diversi, che l'Accompagnanome gliele ristrigne, e gliele determina solamente: là dove l'Articolo, e gliele ristrigne, e gliele determina, e oltr'a questo gliele specifica, e, come da noi conosciuto, il ci pone avanti nel favellare" (pp. 51–52).

Segue un esempio dal *Decameron* (II, 8, 87): con *a guisa di ragazzo* "quasi l'idea del ragazzo esprimerremmo in confuso"; *a guisa d'un ragazzo* "mostra, che chi lo nomina abbia nell'animo una sembianza d'un particolar ragazzo, tuttavia che l'uditore non sappia egli gia

quale"; mentre *a guisa del ragazzo* "n'avrebbe disegnato uno, non solamente da chi lo nomina, ma conosciuto ancora spezialmente da chi sente nomarlo" (p. 52). Ci limiteremo a notare l'anticipazione della concezione moderna di *un* per il nuovo, e di *il* per il noto (e ricordiamo a questo proposito le distinzioni che il Castelvetro nella *Giunta* (1563, p. 11^{v}) faceva, a proposito di *quello* e di *il*, fra tre valori: preterito, futuro, e presente, che possono corrispondere a quelli che oggi si direbbero anaforico, cataforico, e deittico).

Questa categoria di *accompagnanome* viene usata nel *Vocabolario* della Crusca (1612), per indicare uno dei valori di *uno*; e viene ripresa dal Buommattei, *Della lingua toscana* (1643), fra i "ripieni": "Ripieno è una particella, non necessaria alla tela grammaticale: ma serve all'ornamento della frase, per proprietà di linguaggio" (p. 368). Qui si perdono le utili precisazioni del Salviati, ma si aggiunge che questi *uno, una*, non sono numerali corrispondenti al latino *unus, una*, come si vede provando a tradurre in latino gli esempi italiani in cui *uno* è "ripieno". Le stesse nozioni vengono riprese nelle *Regole ed osservazioni* (1745) del Corticelli. Deve essere per influenza della tradizione francese a cui abbiamo accennato (con l'impostazione di Port-Royal, che accosta i due articoli, e il rifiuto dell'indeterminativo ad opera di Du Marsais) che troviamo considerazioni come quelle di R. Zotti nella sua *Grammaire italienne et française* (cito dall'edizione del 1836): "A l'égard des mots *un, une* (uno, una), c'est confondre toutes les notions que de les regarder comme des articles, puisque, s'ils en sont, on sera forcé de donner ce nom à presque tous les autres pronoms, tels que *tutto*, tout; *ogni*, chaque" ecc. (pp. 42–43). E, ancora nel 1841, troviamo nella anonima *Grammatica ideologica o sia le leggi comuni d'ogni parlare dedotte da quelle del pensare* (su cui cfr. Trabalza (1908, pp. 457–462)), che al determinativo viene rifiutato il suo valore generico, e dell'indeterminativo viene negata la possibilità:

"Gli *articoli* pertanto od aggettivi *indicativi* o *determinativi* devono tutti afficere la sola *estensione* de' nomi generali, ed essere *determinativi* di questa, sicchè sarebbe un contrasenso il chiamarne alcuni indeterminati, salvo in quantochè gli uni determinano men precisamente che gli altri, onde a petto di questi pajono *indeterminati*" (p. 201). "L'articolo *il, lo, la* non parmi punto *determinativo d'estensione*, se non quando adoprasi in senso equivalente all'*ille, illa, illud*, latino, ond'ei provenne, quando varrà cioè *quello, quella*, o *la cosa già discorsa, già intesa, indicata* o *nota comunque* [. . .] Non è pertanto vero, che l'*il, lo, la* serva anche per far pigliare il nome comune *in tutta quanta la sua estensione* (*i cani*, quasi valga, *tutti i cani*)" (p. 203). E per l'indeterminativo: "A che pro contendere se *uno* possa essere articolo? certamente che *uno* conformandosi al suo sostantivo notane il genere e numero e colpisce la sola estensione, ma non la comprensione de' nomi [. . .] Pertanto ha sempre in realtà il *valore numerale*, ma senza contraddistinguere, anzi lasciando in incerto a quale individuo fra i molti d'una classe s'applichi, perchè al proposito del discorso ciò non importi [. . .] Siavi pur differenza tra l'*uno* usato rigorosamente come *numerativo* (*Quanti cavalli hai? ne ho uno*), e l'*uno* usato indeterminatamente (*un cavallo mi calpestò; un uomo mi ferì*: dove l'*uno* serve soltanto a notare che l'estensione del suo appellativo è ridotta *ad un solo individuo*, ma ad uno qualunque del vasto genere): ma ciò non fa che non sia sempre numerativo e pel numero singolare; è la voce stessa usata sotto due punti di vista o rapporti, accessori differenti, ma nella sostanza convengono" (pp. 204–206). A quest'epoca, d'altra parte, la battaglia contro l'indeterminativo era ormai perduta. La battaglia, ma, come si è visto all'inizio, non la guerra.

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Old High German *â-*, Old English *ǣ*-: A problem that won't go away

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1. The problem

Throughout the entire Old Germanic speech community we find a prefix that may be reconstructed as **uz-*[1] and occurs in combination with verbs, nouns, and adjectives. Starting from an apparent original (Germanic) meaning 'out' (cf. the identical preposition Gothic *us*, Old Norse *ór, or, ur, yr*, Old High German *ur, ar, ir* 'out of, from'), it has acquired a variety of meanings ranging from the spatial (as in 'go out'), through the actional[2] (as in 'carry out' = 'accomplish'), to the privative or negative (as in 'out of luck' = 'luckless'). This prefix appears in Gothic as *us-, uz-, ur-* (before *r*) and in Old Norse as *or-, ur-, ǫr-* (here predominantly as a nominal prefix, since weakly stressed [verbal] prefixes were generally lost; see Heusler 1967: § 123ff.).

In West Germanic, the situation is far more complex. There are obvious cognates of the North and East Germanic forms: OE *or-*, OFr., OS *ur-, or-* (only in nominal composition), OHG *ur-* (in nominal composition), *ur-, ar-, ir-, er-*[3] (in verbal composition). Beside these, however, there appear prefixes without *-r* but with similar – sometimes seemingly identical – meanings: OE, OFries., OS *ā̆-* in verbal composition, OE *ǣ-*, OFries. *ē*, OHG *â-* in nominal composition. The distribution in Old English and Old High German, the West Germanic languages in which the great majority of these compounds occur, may be illustrated as follows:

Noun-compounds	*or-*	(*or-wēne*	'hopeless')	*ur-*	(*ur-wâni* 'hopeless')
	ǣ-	(*ǣ-bylg*	'anger')	*â-*	(*â-bulgi* 'anger')
Verb-compounds	*ā̆-*	(*a-belgan*	'to anger')	*ar-, ir-*	(*ir-belgan* 'to anger')

This rather neat scheme is somewhat misleading, however. Indeed, even within its confines one is struck by the fact that an Old High German verb in *ar-, ir-* has a corresponding noun in *â-*, not *ur-*; in Old English the reverse is often the case: e.g., *or-þanc* 'original thought, genius, understanding' : *a-þencan* 'to think out, devise' (= OHG *ir-denken*). It is not unusual for nominal compounds in *or-* and *ǣ-* or *ur-* and *â-* to exist side by side, with similar (identical?) meanings; e.g., OHG *ur-herz* : *â-herz*, both glossing *ex-cors* 'foolish'; OE *or-gilde* : *ǣ-gilde* 'without compensation, unpaid for'; or with contrasting meanings, e.g., OHG *ur-werc* '*exercitatio*, exercise, practice' : *â-werc* '*stuppa*, tow oakum (that which is separated out in the course of work)'; *ur-kust* '*astus*, cunning, cleverness' : *â-kust* '*vitium*, fault, sin, wickedness'; *ur-chôse* (Notker) 'indubitable, without contradiction' : *â-chôsunga* (Gl.) '*deliramentum*, senseless talk'.

There are two possible explanations for such an apparently chaotic distribution of forms: (1) all prefixes are somehow derived from the same Proto-Germanic form, **uz-* or something similar, or (2) we are dealing with two originally distinct and unrelated prefixes, the semantic fields of which have largely merged. Either solution has certain drawbacks. The first presents insoluble phonological problems, while the second presupposes the existence of a Germanic prefix **ǣ-* which, it has been claimed, is not only limited to West Germanic, but has no satisfactory etymology.[4]

2. The solution – or so it seemed

Jacob Grimm believed in the original identity of all the prefixes listed above and derived them from earlier **as-* (1870–1878: 1,695f, 781; earlier editions 705, 791). Incidentally, he incorrectly equated OHG *â-* and OE *ā-* (which he considered to be long), noting that in Old High German this form of the prefix occurred only before nouns, in Old English only before verbs; there is no mention of OE *ǣ-*. Although Grimm gave five reasons for his conclusion, all involved meaning and usage; no attempt was made to clarify the phonological muddle. Already Graff (1834–1846: I, 16f.) saw objections to identifying *â-* with *ur-, ar-*, etc., but it was Johannes Schmidt (1883: 41f.) who, with his usual incisiveness and neogrammatical precision, demolished Grimm's conjectures and proposed a possible etymology for the prefix represented in OHG by *â-*, in OE by *ǣ-*. His arguments are worth citing at length:

"Grimm. . . leitet dies *ā* aus *ar-* oder *as-* und identificiert es mit dem as. ags. *ā-*, welches nur in verbalzusammensetzungen erscheint, beides irrig. Gegen die herleitung von *ā* aus *ar* bemerkt schon Graff mit recht, dass *ā* nur in nominalzusammensetzungen steht, welche gar nicht *ar*, sondern nur *ur-* gehabt haben könnten. . . , und hebt begriffsdifferenzen wie *āteilo* expers: *urteil* judicium, *āchōsunga* deliramentum: *urchōsi* elucidum hervor. Das as. ags. *ā-* vor verben ist allerdings aus **az* = ahd. *ar-* entstanden. . . , aber nicht dem ahd. *ā* vor nomina gleich zu setzen."

He then gives examples of the distribution of the prefixes (similar to those given here, above), and proceeds to remark:

"Die beschränkung von *ā-* auf verba, *ǣ-* auf nomina ist in den poetischen denkmälern streng inne gehalten, nur ganz vereinzelt überträgt sich das *ā-* vom verbum auf nebenliegende abstracta. . . Diese differenz von ags. *ǣ-* = ahd. *ā-* und ags. *ā-* = ahd. *ar-* sichert als gemeinsam westgerm. *ā-* (älter *ē*) vor nomina und erweist, dass dies *ā-* nicht aus *ar-*, der unbetonten form zu hochtonigem *ur-, uz-* entstanden ist."

Schmidt also notes for the first time the existence of an ablaut variant of the prefix **ǣ-* in both OE and OHG: **ō-*, for example in OHG *uo-quemo* : *â-quemo* 'descendant'; *ua-wahst* '*incrementum, stirps*, growth, increase, sprout' : *â-wahst* '*recrementum*, weed' (cf. OE *ō-wæstm* 'shoot, sprout'), etc.

He finds parallels in Latin *ā* = *ab, abs* (which is surely incorrect) and in Sanskrit *ā*, which among its many meanings includes that of 'away from' (with ablative); an etymology which was accepted by Walde-Pokorny, Pokorny, and most other standard handbooks. We shall have more to say about this below.

Schmidt's solution, with later minor corrections and additions, has stood ever since as the *communis opinio*, with only two significant challenges, one nearly eighty years ago, and the other quite recent. Since this latest challenge coincided with the preparation of the article '*â-*' for the forthcoming *Etymologisches Wörterbuch des Althochdeutschen*[5] (for which the recipient of this volume serves as a valued consultant), there is a dual reason for reopening the entire question at this time and reassessing the standard view – which is, after all, over 100 years old.

3. The first challenge

W. Lehmann (1906) relied on a massive compilation of forms to demonstrate supposed identity of the prefixes on the basis of usage and meaning, "die Koexistenz des *a-, ur-*, sowie ihre Bindung mit ein und demselben Grundworte" (1906: 34). Only secondarily was he concerned with justifying this conclusion by attempting to find a phonological explanation of the various forms. On the basis of two or three exceptional forms in the Old High German Glosses in which the nominal prefix appears as *ar-* instead of *ur-* and the varying vowels of the Old High German preposition *ur, ar, ir*, Lehmann assumes the existence of nominal compounds with either strongly or weakly stressed prefixes, having developed from combinations of noun plus strongly or weakly stressed preposition, depending on sentence stress. He thus derives OHG *â-* from the weak form of the prefix (*ar-*), with subsequent loss of *r* and compensatory lengthening. OE *ǣ-* is similarly explained, except that *ar* > *ær* before the loss of *r* (Lehmann 1906: 34ff.).

Apart from his highly questionable theory of the origin of noun compounds, Lehmann involves himself in a number of contradictions: (1) the weakly stressed prefix somehow became *â-* in OHG, *ǣ-* in OE before nouns, but *ar-, ir-* in OHG, *a-* in OE before verbs (his explanation: the verbal compounds were formed at a rather late date, at which time the strongly stressed adverb *ur* found itself placed in unstressed position! (1906: 79ff.); (2) loss of *r* with compensatory lengthening, though common in Old English, is otherwise unknown in early Old High German; (3) at the time of the change of Anglo-Frisian *a* > *æ* (according to Luick 1921–1940: § 350, in the 3rd or 4th century), *or* (< **ur*) would not yet have become *ar* in positions of weak stress (according to Luick § 350, this change took place in the 7th/8th centuries). As might be expected, such a phonological explanation did not seriously threaten the prevailing view and has fallen into well-deserved oblivion.

Nevertheless, Lehmann's treatise raises an important issue that should be addressed at this point: to what extent can semantic criteria (in the broadest sense, including usage) be relied on to solve this problem – or similar problems?

Those who wish to prove the ultimate identity of the prefixes stress the fact that there are numerous examples of both *â-* (*ǣ-*) and *ur-* (*or-*) attached to the same word, with similar or identical meanings (see above) and that (rarely) a compound in *or-* in OE may correspond to one in *â-* in OHG (e.g., *or-dǣle* : *â-teili* 'not participating in'); those who believe in two separate prefixes stress the (relatively few) differences in meanings of *â-* (*ǣ-*) and *ur-* (*or-*) compounds (see above). In fact, synonymous doublets such as *ur-herz* : *â-herz, or-gilde* : *ǣ-gilde* could equally well represent alternate forms of the same original prefix (resulting from varying stress patterns?) or the merging of the semantic fields of two originally distinct prefixes. This latter interpretation, to be sure, would best account for the differences

in *â-werc* : *ur-werc*, etc. – differences that Lehmann saw no way to deal with, other than to deny their importance (1906: 34) and ignore individual cases (Lehmann mentions specifically only the contrast *â-teilo* : *urteil*, which he believes he can discredit as evidence in the case [p. 42 fn. 3]).

Perhaps a complete 'field-of-meaning' study of the prefixes, of the sort pioneered by Jost Trier (1931) (see also Lloyd 1961), could reveal further differences, though there is little doubt that most meaning-variants seem to be shared by *â-/ǣ-* and *ur-/or-*. One exception deserves to be mentioned, however. It has long been recognized that *ur-/or-* can be a pure intensifier, without any negative sense, indicating 'to the furthest degree possible'; e.g., OHG *ur-alt*, OE *or-eald* 'very old' (cf. OHG *ir-altên* 'to age'), OHG *ur-mâri* 'famous, outstanding', etc. No such compounds with *â-* (*ǣ-*) are attested. It has been claimed that this function is a later secondary development (Wilmanns 1893–1909: 2, § 417, 3; Lehmann 1906: 31f.), but that seems highly unlikely, since it rests on one of the original basic meanings of Germanic **uz-* in verbal compounds to indicate a finitive actional type (cf. Gothic *us-fulljan* 'to fulfill, fill completely') and is found (contrary to Wilmanns) already in Gothic, where it still shows a connection with an underlying verbal concept; e.g., *us-waurhts* 'righteous, just' as contrasted with *fra-waurhts* 'sinful' (cf. *us-waurkjan* 'to accomplish, achieve').[6] This more positive sense also appears in OHG *urwerc* 'exercise, practice' (cf. *urwercman* '*artifex*, craftsman') as opposed to *âwerc* 'tow, oakum' (see above).

Such differences can certainly be explained most easily if one assumes that *â-* (*ǣ-*) goes back to a particle meaning 'away from, off', which never had an actional function as **uz-* demonstrably did. Thus semantic considerations tend to support the standard view, though they can hardly be said to be conclusive. It seems that it is only in the phonological arena that any definitive outcome is possible, and until recently the Schmidt hypothesis seemed unassailable there.

4. The second challenge

In an article (1979a: 30ff.) Alfred Bammesberger took up the gauntlet, challenging the prevailing view and attempting to demonstrate on the basis of phonological evidence that Grimm was right after all (also briefly in Bammesberger 1979b). His basic arguments may be summed up as follows:

(1) PGmc. **ǣ-*[7], assumed by Schmidt and others, has no satisfactory etymology and is restricted to West Germanic.

(2) PGmc. **ǣ-* cannot be the source of both OHG *â-* and OE, OFr. *ā-*.

(3) PGmc. **ǣ-* cannot be the source of OE *ǣ-*, because then the prefix would appear as *ǣ-* only in West Saxon, *ē-* in the other dialects, which it does not.

(4) Therefore, PGmc. **ǣ-* did not exist.

(5) The "*r*-losen Formen des Westgermanischen" are derived from **uz-*, which became *or-*; then proclitic *o-* became *a-*; *r* was lost with compensatory lengthening.

(6) OE *ǣ-* probably arose by *i/j*-Umlaut, beginning in the numerous "participia necessitatis" of the type *ǣ-bǣre* 'manifest, notorious' (< **uz-bērija-*) and extended by analogy.

(7) Certain parallels can be found in the development of the prefix **tuz-*, which appears both with and without *-r* in OHG *zur-, za-, zi-*, prep. *zuo, za, zi, ze*; OE *tor-, tō̆-*, prep. *tō, te.*

(8) The prefix **ō-* (OHG *uo-*, OE *ō-*) is also derived from **uz-*.

The first of these eight points has some validity. Particles, especially those consisting of a simple phoneme, are notoriously hard to pin down etymologically, and this one is no exception. There are, however, numerous words in every language without satisfactory etymologies, and their existence is not challenged. Let us, therefore, postpone the discussion of the etymology of **ǣ-* until later, and first dispose of the arguments against its existence.

First of all, Bammesberger has set up a straw man and then proceeded to knock him down. By throwing together the OE, OFr., OS verbal prefix *ă̆-*[8] and the nominal prefix OHG *â-*, OE *ǣ-*, he can safely say (1979a: 32f.) that these cannot all be traced back to "WGmc. *ā-*", i.e., PGmc. **ǣ-*. It is doubtful if anyone today would make such a phonologically unjustifiable claim; certainly Schmidt did not. He clearly stated (Schmidt 1883: 41; see also above) that OS, OE *a-* before verbs = OHG *ar-* and has nothing to do with OHG *â-*, OE *ǣ-*.[9]

Secondly, Bammesberger is quite correct in stating that "WGmc. **ā*" occurs in OE as *ǣ* in West Saxon and *ē* elsewhere. The lack of *ē-* would indeed be unexplainable; however, there is no lack of *ē-*. Since most extant Old English manuscripts are either West Saxon or strongly influenced by West Saxon (see Sievers-Brunner 1965: § 2, especially Anm. 6 and 7), it is no surprise that the overwhelming majority of the occurrences of the prefix have the form *ǣ-*, but even a cursory check of Bosworth-Toller (1898–1921) and Wright-Wülcker (1884) reveals the following compounds with *ē-* beside the more usual *ǣ-* (dialects are noted where fairly certain): *ē-bylgđu* (Mercian), *ē-cambe* (?), *ē-mōd* (?), *ē-spryngum* (Mercian), *ē-swic* (Mercian), *ē-uuerdlu* (Northumbrian), *ē-wyrdlu* (Mercian). As far as I can tell, the occurrences of *ǣ-* are in MSS with at least some West Saxon traits.

There is thus no reason to deny a PGmc. **ǣ-* as the source of OHG *â-*, OE *ǣ-* (*ē-*) – and also OFr. *ē-*, which was not mentioned by Bammesberger, but occurs in the compounds *ē-bēte* 'unatoned' ('*busslos*'), *ē-felle* 'without skin or covering' (cf. OE *ǣ-felle*), *ē-frēthe* 'without paying Friedensgeld' (a fine paid to the authorities; see Hoops 1911–1919: 2, 96ff.), *ē-lĭve* 'lifeless'.[10] In fact, as we shall see, no other source can plausibly be posited.

Bammesberger's fifth point is one I would agree with completely, if by the "*r*-losen Formen des Westgermanischen" we understand the verbal prefix *ă̆-* in Old English, Old Frisian, Old Saxon.[11] For Old English, each step of the development represents a standard phonological change:

(1) **uz* > **ur* (standard West Germanic rhotacism; Sievers-Brunner § 181, 2).

(2) *ur* > *or* (*u* > *o* before *r* < *z*; Sievers-Brunner § 45, 5).

(3) *r* was lost in monosyllabic words (Sievers-Brunner § 182). This loss, which was standard in Old English, Old Frisian and Old Saxon – the languages that have a verbal prefix without *r* – did not occur in (early) Old High German – which has a verbal prefix with *r* –; compare for example OE *mē* : OHG *mir* 'to me'; OE *hvā* : OHG *hwer* 'who'. This is another reason to rule out the derivation of OHG *â-* from **uz-*.

After the loss of *r*, the remaining vowel was lengthened in words that could be used in strongly stressed positions, but probably remained short when such words occurred with weak stress (Sievers-Brunner § 137,1 and Anm. 1). The length of the vowel in the verbal prefix *a-* is argued. Since the loss of *r* must have occurred while **or* was still an independent preposition, able to appear in various stress patterns (cf. OHG *ur, ar, ir*), it is possible that both long and short vowels occurred, but since verbal prefixes were regularly weakly stressed, I would agree with Lehmann (1906: 80, n. 2) in positing a short vowel for such prefixes.

(4) **o-* > *a-* in positions of weak stress (Luick 1921–1940: § 323).

While this explanation deals nicely with the verbal prefix *a-*, it cannot be applied to OHG *â-*, as we have seen, nor does it yield an OE *ǣ-*. Bammesberger's solution to the problem of this troublesome form is ingenious and even theoretically possible, though it does require considerable analogical extension to forms that contained no umlaut-producing sounds. One might ask, however, why such an umlaut occurred only in nominal compounds; a number of compounds in *a-* with first-class weak verbs are equally capable of umlaut (e.g., *a-firran, a-lȳsan, a-lecgan, a-þencan*, etc.); or why the numerous nominal *or-* compounds of the type *or-dǣle* did not generate umlaut of this prefix. One could extend such analogies at will. Obviously the fact that umlaut did not take place in other similar compounds does not rule out the possibility that it did, for whatever reason, in *ǣ-* compounds. This is the sort of speculation that cannot be disproved; however, it is fortunately unnecessary in order to explain the Old English prefix *ǣ-*.

Bammesberger's final two points are in fact rather peripheral to the central argument, and a detailed discussion of the myriad problems connected with the prefix **tuz-* and other related and non-related forms could provide the matter for another entire article. Suffice it to say that at least two and possibly three different word groups have been thrown together here. To single out the one nearly certain thing in the mass of uncertainties surrounding these words, OHG *zuo*, OE *tō* 'to', both also used as prefixes, are totally unrelated to OHG *zur-*, OE *tor-* (nominal prefix) 'bad, difficult, asunder', OHG *zar-, zir-, za-, zi-*, OE *tŏ-, te-* (verbal prefix) 'apart, asunder'. Whether in turn OHG *zur-*, OE *tor-* are really identical to OHG *za(r)-, zi(r)-* (the *r* may be secondary, from *ir-*), OE *tŏ-, te-*, or whether the former are connected with Gothic *tuz-*, Greek δυσ- and the latter with Lat. *dis-*, is impossible to say for sure.[12] In any case, this group of words provides rather shaky ground on which to erect any edifice. Moreover, I fail to see any real parallel to *ur-* : *â-*. Even if *zur-/tor-* is the stressed equivalent of *za(r)-/tŏ-*, it consistently retains the *r*, just like *ur-/or-*, and never loses it to yield a form corresponding to *â-/ǣ-*. The usual loss of *r* in the Old High German verbal prefix *za-, zi-* does not agree with its consistent retention in OHG *ar-, ir-*.[13]

Finally, not only is the attempt to derive the prefix *uo-/ō-* from **uz-* phonologically questionable, especially for Old High German, where no parallel exists for early loss of *r* with compensatory lengthening, but Bammesberger also fails to explain why the same nominal prefix **uz-*, which already is supposed to appear sometimes as *ur-/or-*, at other times as *â-/ǣ-*, sometimes takes on still a third form *uo-/ō-* – even alternating with *â-/ǣ-* in the same word, as in OHG *â-quemo* : *uo-quemo*. Here varying stress patterns simply won't work.[14]

Since not a single argument against the derivation of the prefix OHG *â-*, OE *ǣ-* (*ê-*), OFr. *ê-* from PGmc. **ǣ-* can be upheld, and the arguments for its derivation from PGmc. **uz-* are phonologically implausible, there would seem to be no reason to question the existence of a PGmc. particle **ǣ-*, as accepted in most handbooks.

5. The etymology of PGmc. **ǣ-*

To return to the first point raised by Bammesberger, there is no doubt that etymological support for Gmc. **ǣ-* is weak. It is true that the particle can be identified only in West Germanic, where, it is interesting to note, it seems to be in decline, eventually dying out in all languages, with the exception of a few isolated remnants like NHG (dialectal) *Öhmd* 'aftermath, second mowing' (< *â-mâd*). It is totally lacking in Gothic. In Old Norse, the situation is not so simple. If the prefix had survived, it would have fallen together with the prefix *á-* = Gothic *ana-*, German *an-*, and indeed in some environments with *á-* < *af-* (see Noreen 1923: § 299,5; Cleasby-Vigfusson 1957: 5 [s.v. *af*, D]), which could well have been sufficient to hasten its final disappearance. G. Schmidt (1962: § 176) believes remnants of an original *á-* < **ǣ-* can still be identified in a few compounds; he lists *á-máttigr*, an epithet of witches and giants, which he interprets as 'with evil power' (Cleasby-Vigfusson [1957: 43] even compares OHG *â-mahtig*, which, however, means 'powerless'), *á-verk(i)* 'unlawful work', *á-vǫxtr* 'fruit, produce, growth' (OHG *â-wahst* means 'weed', but compare the ablauting *uo-wahst* 'growth, increase, sprout'), *á-ván, á-væni* 'faint expectation' (cf. OE *ǣ-wēn* 'doubtful, uncertain'), *á-munr* 'eager (for revenge)' (contrast *á-munr* 'like, similar' < **ana-*).

None of these must be traced back to an original **ǣ-* – indeed, I have my doubts about a number of them –, but they indicate that we simply have no way of knowing whether Old Norse once had such a prefix or not, and that its supposed isolation in West Germanic may not have been so complete.

Before leaving the Germanic languages, a word must be said about the prefix that appears in OHG as *uo-* and in OE as *ō-*. As we have seen, it can have the same meaning as **ǣ-* (*âquemo* : *uoquemo*) or a different meaning (*âwahst* : *uowahst*). Attempts by Lehmann (1906) and Steinhauser (1960) to demonstrate that **ō-* must be unconnected with **ǣ-* on the basis of semantic differences are not fully convincing. To be sure, the few examples of **ō*-compounds that we possess show a smaller range of meanings than those with **ǣ-* – there are no examples of **ō-* with a purely negative meaning, for example, unless OS *ōbult* belongs here (see below) –, but **ō-* expresses no meaning not shared, or at least approximated, by *ǣ-*. Typical meanings of **ō-* are: 'down (away from)' as in OHG *uo-hald* 'precipitous, sloping down'; 'after, later' as in *uo-quemo*; and (closely related to this meaning) 'added, supplementary' as in OE *ō-wæstm*, perhaps also in *ō-web* '*subte(g)men*, woof (in weaving)'. All of these meanings can easily be derived from 'proceeding away from', and the last two can be compared with the use of **ǣ-* in *â-quemo, â-mâd*. While such evidence is not sufficient to prove anything conclusively, the existence of two such similar particles certainly suggests the possibility of ablaut variants, as J. Schmidt claimed. Unless a better explanation can be found, this can at least be accepted as a working hypothesis.

The particle **ō-* is quite rare, with certain occurrences only in Old English and Old High German.[15] It may be found in Old Saxon in two words: *ōbult* 'anger' (Wadstein 1899:

52.212) and *ōlat* 'thanks' (*Heliand*, with variant *ālat*). The first may equally well be an error for **orbul(h)t* (cf. Holthausen 1921: § 227 Anm.), while the second has no sure etymology, but has been connected with Gothic *awi-liup* 'thanks' (Feist 1939: 70; Berr 1971: 308).[16] The often cited MLG word *ō-herde* (incorrectly listed by Steinhauser [1960: 101] as Anglo-Saxon), usually translated 'Unterhirt', is probably actually 'sheep-herdsman', from *ouwe, owe* 'sheep', as in the compound *ō-hōf* 'sheep farm' (Schiller-Lübben 1875–1881: 6, 226f.). There is no sign of the prefix in Old Frisian, or in Gothic. In Old Norse, there is again no way to determine whether the prefix was present, since it would have fallen together with the much more common negative prefix *ó-* < **un-* 'un-' (see Noreen 1923: § 112,1). Either this would have resulted in contradictory pairs (**ó-hallr* 'precipitous' < **ō-* : *ó-hallr* 'not sloping' < **un-*), or, if **ō-*, like **ǣ-* ever developed a negative meaning, this function could have been taken over by *ó-* < **un-*[17] (ON *ó-bœttr* 'unatoned' is synonymous with OFr. *ē-bēte*).

The most obvious cognate to Gmc. **ǣ-* and **ō-* in non-Germanic languages is the ubiquitous Indo-Iranian preposition and postposition (with accusative, locative, ablative) and prefix *ā(-)*, which has a large array of meanings: 'to, up to; in, among; in addition to; somewhat; away from'.[18] It has been suggested that all these meanings arose from the basic idea of 'near' (Pokorny 1959: 280; Steinhauser 1960: 103), which, in order to arrive at 'away from', would apparently require a development via 'in the vicinity of, but no longer at'. This is certainly possible; the history of particles is frequently one of wide-ranging semantic change. As particles develop less specific functions, however, there is always the possibility of merger of similar particles, and this could also be the case here. One could theoretically posit a merger of particle$_1$ with a basic meaning 'to, at, in' or the like, and particle$_2$ 'away from', which could easily overlap in borderline areas such as 'near', 'somewhat'. I leave it to specialists in the Indo-Iranian languages to choose which alternative best fits the history of the particle in those languages.

In any case, Indo-Iranian *ā* can represent PIE **ā, *ē*, or **ō*. The Germanic particle **ǣ-* can only come from PIE **ē*; the particle **ō-* could come from either **ō* or **ā*, though an ablaut pair **ē-* : **ō-* is more probable. Moving beyond Indo-Iranian, we find in the Slavic languages a rather rare 'colorless' prefix *ja-*, as in Serb.-Church Slav. *ja-skudъ* beside *skǫdъ* 'ugly(?)',[19] which may be related – and which again could come from PIE **ā, *ē*, or **ō*. Evidence for **ō* may be provided by one or two Greek words: *χηρωσταί* 'collateral relatives who inherit for the lack of closer relatives' (**χῆρον 'verwaistes Gut'* + **ō-d-ta*; cf. Skt. *ā-dā-* 'receive', ppl. *ā-t-ta-* 'received'; so also Frisk 1960–1972: 2, 1096f.; Mayrhofer 1953–1980: 1, 69; opposed Steinhauser 1960: 104) and possibly *ὠ-χρός* 'yellowish, pale' (cf. the use of the Sanskrit prefix *ā-* in such words as *ā-piṅgala-* 'yellowish'), though this etymology is doubted by both Frisk (1960–1972: 2, 1153) and Mayrhofer (1953–1980: 3, 274). Since a connection between Gr. *ὠχρός* and Skt. *vyāghrá-* 'tiger' is highly unlikely, the second element of Gr. *ὠχρός* is left totally isolated.

Evidence for **ē* is very weak outside Germanic: perhaps ablauting with the *ō* in Gr. *χηρωσταί* is the *ē* in Lat. *herēs, -ēdis* 'heir' (< **ĝhēr-ē-d-* ?; accepted by Walde-Hofmann 1938–1956: 1, 641f.; Frisk 1960–1972: 2, 1096f.; opposed Steinhauser 1960: 111) even more uncertain in Gr. *ἠβαιός* beside *βαιός* 'little' (opposed by Frisk 1960–1972: 1, 619; uncertain Schwyzer 1953: 2, 491). There is no specific evidence for **ā*. Attempts to connect various particles with the short vowels *e* and *o* as ablaut variants are all uncertain to varying degrees (see Pokorny 1959: 280f.).

The overall etymological evidence is not as strong as one might wish, but at least there seems no reason to question the connection of the Germanic particles with Indo-Iranian *ā*(-), which is both semantically and phonologically plausible, whether one chooses to derive the Indo-Iranian particle from PIE **ē* or **ō*, or even from a coalescence of two ablaut variants or two originally distinct particles that came to be felt as ablaut variants. While all such details may never be entirely clarified, the central fact of the existence of the Germanic particles **ǣ-* and **ō-* remains as firmly established today as at the time of Johannes Schmidt.

Notes

[1] Probably from PIE **ud-s*, a derivative of the particle **ud-* (Skt. *úd-*, *út-* 'up[ward], out'; with long vowel Gothic, OE *ūt*, OHG *ûz*), with an *s*-determinative (as also probably in the synonymous Skt. *us-*, Avestan *uz-*). See Walde-Pokorny (1927–1932: 1, 189f.); Pokorny (1959: 1103f.); Feist (1939: 528); Kluge (1975: 808) (inconclusive).

[2] For a discussion of 'actional types' – similar to what are called *Aktionsarten* in German, see Lloyd (1979: 91ff., 103f. [finitive function of Gothic *us-*]).

[3] *ur-* is early and rare, *er-* is late Old High German and Middle High German. The 9th-century East Franconian form *ir-* is generally used in examples, unless a specific form is cited.

[4] For a complete list of OHG *â*-compounds see Karg-Gasterstädt (1952–: 1, 1). OE *ǣ*-compounds have been collected from various sources, including Bosworth-Toller (1898–1921) and Lehmann (1906).

[5] Edited by Albert Lloyd and Otto Springer and supported by grants from the National Endowment for the Humanities and the William Penn Foundation.

[6] Of course the further analogical extension of this prefix to such words as OHG *ur-âno* 'ancestor', NHG *Ur-wald* 'primeval forest', etc., building on such compounds as *ur-alt*, is secondary, as is the development of the meaning 'original, proto-'.

[7] Bammesberger refers throughout to "West Germanic **ā*", a formulation that is of course phonemically every bit as proper, but which, for phonetic reasons, I prefer to avoid. The development of PIE **ē* to OHG, OS *ā*, OE *ǣ*, OFr. *ē* presupposes a PGmc. rather low front vowel in the general area of [æ:] but with higher and lower allophones.

[8] The length of the vowel is disputed; see below.

[9] Bammesberger (1979a: 31, n. 5) misstates Schmidt's position.

[10] See Helten (1907: 88, 89, 94). The prefix *ă̆-* in the noun compounds *ă̆bēl* 'a raised scar', *ă̆bēre* beside the more common *aubēre*) 'manifest', and *ă̆dēl* 'legate' (? listed only in Holthausen 1925: 1) has been explained variously as an error or an analogical transfer of the verbal prefix. See Helten (1907: 24, 28).

[11] Lehmann (1906: 79f.) also came to a somewhat similar conclusion, as did Siebs (1899: § 101, Anm. 2) for Frisian. It is also verified by the rare survival of the full prefix *ar-* before a vowel in OE ***ar-æfnan*** 'perform', "falsely divided to alliterate on *r*, and giving rise to a simplex ***ræfnan***" (Campbell 1959: § 73 fn. 2).

[12] On the etymology of OHG *zuo*, OE *tō* < PIE **dĕ̆* : **dŏ̆*, see Kluge (1975: 889), Pokorny (1959: 181ff.). On the problematic relationship of the prefixes, see Braune-Eggers (1975: § 72, Anm. 2), Lehmann (1906: 80ff., n. 2); Kluge (1975: 880) (merges OHG *zur-* with *za[r]-*, *zi[r]-*, with no mention of OE *tor-*). Most handbooks separate the two sets; e.g., Feist (1939: 119 [s.v. *dis-*], 484 [s.v. *tuz-werjan*]); Holthausen (1963: 350 [*tŏ̆-*, *te-*], 351 [*tor-*]); Pokorny (1959: 232); Walde-Hofmann (1938–1956: 354f.); Frisk (1960–1972: 1, 425).

[13] The much less common Old High German forms with *-r* were limited to Upper German; only in Middle High German did *r*-forms become more frequent, in New High German entirely supplanting the others (see Braune-Eggers 1975: § 72 and Anm. 1, 2). The different behavior of *ar-* and *za(r)-* has not been satisfactorily explained. G. Schmidt (1962: § 312) plausibly suggests that it may result from the existence of the former as an independent preposition (regular retention of *-r* in monosyllables with short vowels), as contrasted with the sole use of the latter as a weakly stressed prefix (loss of *-r* – or perhaps already **-z* – in pretonic position?).

[14] I do agree with Bammesberger, however, in connecting *uo-/ō-* with *â-/ǣ-*, contrary to the views of Lehmann (1906: 138ff.) and Steinhauser (1960).

[15] Since the occurrences are so limited, a complete list might be useful (Old High German citations are from my files, Starck-Wells (1972–), and Schützeichel (1981); spellings are normalized; Old English citations rely on Bosworth-Toller, so completeness is not guaranteed):
OHG: *uo-hald(i), -îg* 'precipitous, sloping down'; *uo-haldî* 'precipice, steep slope'; *uo-kalo 'recalvaster,* bald in front, with a high bald forehead' (perhaps actually 'balding'; cf. Skt. *â-* 'somewhat', and note that Lat. *recalvus*, closely related to *recalvaster*, can mean 'bald in front' or 'bald in back'!); *uo-kumft* 'succession'; *uo-kumil(o), -quemil(o), -kumiling* 'late-ripening grape'; *uo-quemo* 'descendant'; *uo-staft 'pittacium*, patch'; *uo-wahst* 'growth, increase, sprout'; *uo-zurnen, -zarnen* 'to disdain, scorn' (from a noun **uo-zorn*?). The often cited OHG **uo-mâd* does not exist; see Steinhauser 1960: 101.
MHG: *uo-sedel 'vectis*, lever or bar'; *uo-sezzel* (?) 'something to cover a hole in a cloak' (*Lanzelet* 6023; see Lexer 1872–1878: 2, 1998; Benecke-Müller-Zarncke 1854–1861: 2, 2, 339; the MS has *vosezzel*, which could also stand for **vor-sezzel*. See also Pfeiffer *Germania* 3 (1858): 480, who suggests **für-fezzel*; unlikely). OE: *ō-den* 'threshing floor'; *ō-gengel* 'bolt (for a door)'; *ō-heald, -hilde* 'precipitous, sloping down'; *ō-leccan* 'to flatter, be obsequious', *ō-leccere* 'flatterer', *ō-leccung* 'flattery, allurement' (all probably from an original lost noun-compound; cf. also *ǣ-lecung* 'allurement'; I can find no trace of an OS *ō-leccan*, cited by Steinhauser 1960: 102); *ō-wæstm* 'sprout, shoot'; *ō-web, -wef 'subte(g)men*, woof'.
Steinhauser (1960: 102) also connects Old High German proper names in *Ô-*, such as *Ô-gast, Ua-gast, Ô-hilt(a), Ô-munt, Ô-reht*: the etymology of such names is, however, extremely uncertain.

[16] Steinhauser's (1960: 102) connection with MHG *letzen* is improbable, since the basic meaning of this word (as also OHG *lezzen*) was 'hinder' (cf. Engl. *let*). A direct connection with OS *lâtan* 'let, allow, leave' suggests itself immediately, but presents severe semantic difficulties ('a farewell gift'?).

[17] Compare such Old High German synonyms as *â-kust* and *un-kust, â-mahtîg* and *un-mahtîg.*

[18] See Böhtlingk-Roth (1855–1875: 1, 581ff.); Monier-Williams (1899: 126). Unless otherwise specified, the following etymological data are from Walde-Pokorny (1927–1932: 1, 95f.) and Pokorny (1959: 280f.). See also the forthcoming *Etymologisches Wörterbuch des Althochdeutschen*, s.v. *â-*.

[19] Rozwadowski (1909: 102); cf. Old Church Slav. *skǫdъ* 'needy, wanting', but Polish *pa-skudny* 'foul, nasty, ugly'.

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Quelques noms grecs à l'Agora d'Athènes

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En 1976, Mme Mabel Lang a donné une excellente publication des graffites et dipinti qui ont été rassemblés à la suite des fouilles américaines de l'Agora[1]. La consultation de ce travail fournit l'occasion de revenir sur un certain nombre d'anthroponymes plus ou moins rares.

Je signalerai tout d'abord à l'attention le seul document qui, quoique pleinement grec, n'est pas rédigé en alphabet, à savoir un petit graffite incisé sur le pied d'une kylix à vernis noir (deuxième partie du Ve siècle). Fait notable, mais pas trop surprenant, il s'agit de cinq signes syllabiques chypriotes, no. F 67 (Plate 13, dessin). L'éditeur a correctement dessiné et transcrit: *ku-po-ro-ta-mo* ou Κυπροδάμω. Gravure en syllabaire commun, de droite à gauche, avec des signes assez réguliers (selon le dessin); noter seulement, à gauche, un *mo* de forme presque carrée[2]. On doit alors ajouter que le composé Κυπρό-δαμος, parfaitement formé, est nouveau, et vient s'ajouter à la riche série des composés constitués avec un premier élément Κυπρο-, série qui est naturellement caractéristique pour Chypre[3].

D'ailleurs, la présence de Chypriotes en Attique est déjà attestée par quelques témoignages, grâce à des noms de dédicants ou de défunts, ordinairement transmis en alphabet. On citera ainsi les Salaminiens de Chypre Ἑλλομένης (fils) et Ἑλλαγόρας (père), dans une épitaphe du Pirée, IG II^{2}, 10217/8 (après le milieu du IVe s.)[4]. Ou encore, un homme originaire de Marion, souvent cité[5], Πυντόδαμος Στασαγόρου, en IG II^{2}, 9284. Etc.

Mais ce qui est très remarquable pour le graffite de l'Agora, c'est l'utilisation du syllabaire chypriote en Grèce propre: on a ici le second exemple connu, après la découverte assez récente d'un pied de lion en bronze de Delphes, incisé avec quelques signes[6], avec la différence que cet objet, appartenant à un trépied, a sûrement été importé de Chypre.

J'examinerai ensuite quelques noms qui me paraissent dignes de commentaire, en suivant simplement l'ordre alphabétique.

Βλόσυς, sur un fragment de pithos du VIe s., D 29, Pl. 8. Ce nom ne serait pas connu, du moins en Attique; effectivement, il manque dans les répertoires usuels. En fait, il est attesté à Abdère, et pourrait bien représenter un nom ionien, quelle que soit l'origine du personnage à l'Agora. Une émission rare de monnaies d'Abdère du Ve siècle[7] porte la légende très claire ἐπὶ Βλόσυος: on sait que, sur de nombreuses séries d'Abdère et de Maronée, comme à Byzance, le nom du magistrat monétaire est très souvent indiqué grâce à la formule ἐπί plus le génitif[8]. La forme Βλόσυς est évidente, mais comment l'expliquer? Elle ne paraît pas avoir attiré l'attention de Bechtel. Il me semble, en tout cas, qu'on pourrait l'interpréter comme un diminutif en -υς[9] de l'adjectif homérique βλοσυρός, lequel signifie probablement

"au regard terrible", selon l'analyse de Leumann[10]. L'existence d'un terme de ce genre comme sobriquet n'aurait certes rien de surprenant. Un autre nom rare Βλόσων est probablement apparenté[10a].

Ἐπόνασος, sur l'anse d'un petit canthare à vernis noir, daté vers 520–490, G 4, Pl. 30. Ce nom figure dans une dédicace archaïque à Hermès, Ἐπόνασος ἑρμεῖ[11]. Le premier nom est curieux, mais paraît bien lu, et il ne semble pas qu'on puisse comprendre autrement le début avec un nom simple plus connu, soit ΕΠ(?) Ὄνασος. L'éditrice, sans commentaire, a vu ici un composé nouveau Ἐπ-όνασος. Il s'agit alors du groupe des composés en Ὀνασι-, qui ne fournit pas, en règle générale, de second élément de composé en *-όνασος/-όνησος*[12]. Cependant, il se trouve que j'ai commenté naguère l'apparition d'un composé fort rare de ce type, à savoir Μορτ-όνασος dans une inscription de Théra, IG XII Suppl. no. 67 (épitaphe, Ve siècle)[13]. Ce nom doit représenter le "renversement" des éléments constituant le composé banal Ὀνασί-μβροτος; dans le cas d' Ἐπ-όνασος, il faudrait plutôt songer à l'influence, dans l'onomastique d'une famille, de l'un des très nombreux composés en Ἐπ(ι)-[14]. On notera enfin que le nom, avec un *a* long conservé, ne peut guère être attique[15].

Εὔπραχσις, sur un fragment de grande amphore du début du VIe siècle, D 7, Pl. 7. Lettres archaïques, graphie χσ. L'intérêt de ce graffite est de fournir un exemple ancien du nom de femme Εὔπραξις, que Bechtel ne pouvait pas citer avant le IVe siècle[16]. Ce nom, très clair, est simplement le substantif abstrait *εὔπραξις* "bonheur" (variante, employée par Eschyle, du banal *εὐπραξία*), et se rattache à une catégorie très riche de noms féminins; à l'époque impériale, les noms Eupraxia et Eupraxis sont encore en usage à Rome[17].

Μάλικος, sur l'anse d'une jarre tardive, V^{e} ou VIe siècle de notre ère, F 325, Pl. 28. Graffite Μαλίκου, assez légèrement gravé. L'éditeur évoque un nom Μάληκος, attesté en Attique (notamment PA 9661, Acharnien, dans IG II^{2}, 5816). J'ai étudié en détail ce nom ancien et rare, qui apparaît à Corinthe dès le VIIIe ou VIIe s. avant notre ère, et a été employé dans diverses régions, toujours avant notre ère[18]. Je doute qu'on puisse le retrouver si tard à l'Agora. A l'époque de ce fragment, je verrais plutôt ici une variante du nom sémitique assez banal Μάλιχος, attesté en Syrie, à Palmyre, en Egypte, etc[19]; une variante Μάλικος précisément connue à Palmyre[20]. D'autre part, le nom est attesté en Attique, au moins sous l'Empire, à en juger par une épitaphe du Laurion, IG II^{2}, 12029, qui porte Μάλιχε χαῖρε[21].

Ξανθῆς. Sur un tesson venant d'une amphore non vernissée, datable vers 490–450, D 39, Pl. 9, on trouve toute une liste de noms au nominatif, cinq masculins, deux féminins, soit: Μενέδημος, Χαρίας, Ξανθῆς, Ἀμφίβουλος, Πρώταρχος, Πενταρίστη, Καλ(λ)ιστράτη. La plupart de ces noms n'appellent pas de commentaire spécial. Cependant le troisième, Ξανθῆς, est notable comme un bon représentant de la catégorie connue des noms ioniens où *-ῆς* résulte de la contraction de *-έας*. En face de Ξανθέας, on a donc Ξανθῆς, déjà attesté mais plus tard, en domaine ionien[22].

Πενταρίστη, même liste, sixième nom. Ce féminin est à première vue bizarre, car l'on attend Πανταρίστη; cependant, l'epsilon est certain, et un masculin similaire Πεντάριστος est attesté plus tard à Cyrène[23]. D'autre part, une stèle attique (Paiania) du début du IVe s. offre la double légende Πενταρίστη Πανταρίστη selon l'éditeur[24]. D. Robinson voyait ici deux noms de femme différents, pour deux personnages. Le second serait le nom normal Πανταρίστη. Mais le premier serait autre chose, avec cette curieuse explication:

"Πενταρίστη is possibly the name of the child whose mother may have died in child-birth. She may have been the fifth child and so called the fifth best (sic), or the fifth of the Ariste family. The name is new, but it is a perfectly possible formation. . ."[25]

Une telle analyse est impossible, et l'on sait qu'il n'existe pas de noms constitués avec un premier élément Πεντ(ε)- "cinq". D'autre part, le voisinage des deux orthographes sur l'épitaphe est remarquable. Je ne saurais dire s'il s'agit ici réellement de deux femmes différentes, appartenant à la même famille, ou plutôt de deux variantes, le même nom étant écrit sous deux formes. En effet, j'estime que Πενταρίστη, à côté du Πεντάριστος cyrénéen, représente une variante rare et sporadique, dans laquelle le premier epsilon résulte simplement d'une dissimilation phonétique des deux *a* consécutifs. Un parallèle en Attique serait fourni par la graphie Ψεμάθη à côté de Ψαμάθη[26]. Ainsi, tout rentre dans l'ordre, et l'exemple de l'Agora, au début du Ve s., est intéressant par sa date.

Πραχσ(ί)νη, sur un fragment de grande amphore, VIIe/VIe s., D 10, Pl. 7. L'interprétation comme Πραξίνη, soit le féminin de Πραξῖνος, est évidente. On remarquera la date haute et l'absence d'iota qui doit résulter d'une graphie rapide, bien admissible sur un graffito[27].

Πύχσων(?). Une lekane du Ve s., C 21, Pl. 5, porte des noms et l'acclamation καλός, inscrits négligement dans un quadrillage. A la l. 3, l'éditeur suggère de lire Πύχσωνος, vraisemblablement comme génitif. Si cette interprétation est correcte, je me demande si l'on n'obtient pas ainsi un sobriquet nouveau, Πύχσων = Πύξων, tiré de πύξος "buis". Comme on sait, Bechtel a relevé plusieurs dérivés en -ων de ce type, comme Δάφνων, Δρύων, Ἐλάτων, Μύρσων, etc[28].

Σικέλα. Sur la base d'un skyphos du troisième quart du Ve s., C 27, Pl. 6, on a le graffite Σικέλα καταπύγ(αινα), qui relève d'un schéma bien attesté[29]. Il faut noter seulement le nom Σικέλα, tiré de l'ethnique correspondant, car il était inconnu de Bechtel, pour le masculin comme pour le féminin[30].

[Σ]ισυ(μ)βρῆς. Fragment inscrit, base d'une coupe à vernis noir, second quart du Ve s., F 84, Pl. 14. Sept lettres régulièrement incisées. Avec une restitution plausible à gauche, l'éditeur écrit Σ]ισύ(μ)βρες et remarque:

"The eta (written as epsilon) following rho suggests a foreigner. The nearest parallel for this name is Σισύμβριον (nick)name for an hetaira in Theophilos' Flute-lover (Edmonds. . . , fr. 11)"

Effectivement, avec une lettre de plus et la non notation de la nasale[31], il est tentant de retrouver un tel nom. Cependant, on écrira plutôt Σισυμβρῆς, avec une terminaison ionienne du même type que Ξανθῆς étudié ci-dessus. D'autre part, on peut citer aujourd'hui des exemples supplémentaires de surnoms de cette famille, formés sur σισύμβριον "menthe aquatique" ou "cresson"[32]. Bechtel lui-même aurait dû signaler les noms masculins Σισυμβρᾶς et Σισυμβρίσκος, employés pour le même personnage chez Hérondas, Mime II, 76. Enfin, il ne faut pas oublier que ces surnoms ne sont pas nécessairement péjoratifs: à Athènes, on relèvera le masculin probable Σίσυμβρις, IG II^2, 12615 (majuscules d'après Ross "litteris pulcherrimis", sans doute IVe s.).

Σύβαρις. Sur un fragment de tuile, "graffito on the glazed side, obviously written on the sherd", début du VIe s., D 6, Pl. 7. En lettres sinistroverses, on lit: Γοργίας ho Συβάριος[33]. Probablement ici un Gorgias fils de Sybaris, comme l'entend l'éditeur, avec un métronyme ("mother's name"), ce qui fournirait un nouvel emploi ancien de ce procédé[34], plutôt

qu'avec un ethnique irrégulier de Sybaris[35]. On sait, en effet, qu'il a existé un nom de femme Σύβαρις, et que Thémistocle l'avait donné à l'une de ses filles, les autres étant dénommées Ἀσία et Ἰταλία, suivant un passage de Plutarque, Thémist. 32[36]. L'exemple épigraphique de l'Agora serait à ajouter au dossier.

Τριβαλ(λ)ός. Sur une base de lekane, graffito partiellement lisible, du deuxième quart du Ve s., F 62, Pl. 13. Il doit s'agir de l'ethnique des Triballes, écrit sans gémination du lambda, déjà attesté comme nom d'esclave[37]; ici, le statut du signataire est impossible à déterminer.

Τυρσανός. Sur la base d'un lécythe, début du Ve s., F 44, Pl. 12. On a ici un exemple ancien et intéressant de l'ethnique des Tyrsènes, employé comme nom d'homme; Bechtel ne pouvait citer que le féminin plus récent Τυρρηνίς[38], et l'on a aussi Τυρρηνός, par exemple SEG XXVII, 735 (IIe s. de notre ère). On notera ici la graphie non attique Τυρσα-[39].

Φαίσστιος. Sur un skyphos à vernis noir, du second quart du Ve s., F 77, Pl. 14. Légende au génitif de possession: Φαισστίου, avec redoublement graphique du sigma[40]. C'est donc un exemple ancien de l'ethnique de Phaistos, utilisé comme nom. Il ne figure pas encore chez Bechtel, qui mentionnait Φαῖστος, forme identique au toponyme[41], ainsi que le dérivé Φαιστιόννας particulier à la Crète[42]. Mais pour le nom du père du célèbre Epiménide, la tradition hésite entre Φαίστιος et Φαῖστος[43]. La signature de l'Agora montre que la première forme est parfaitement plausible[44].

Notes

1 Mabel Lang, The Athenian Agora, Vol. XXI, Graffiti and Dipinti, Princeton, 1976. Pour des raisons typographiques, la graphie des formes attiques a été modernisée ici en ce qui concerne les voyelles longues (emploi de êta et oméga).

2 Document repris chez O. Masson, Les inscriptions chypriotes syllabiques, seconde édition augmentée, 1983, no. 369b, fig. 154. Pour les signes du syllabaire commun, ibid., fig. 1–2.

3 O. Masson, ibid. 413, index, et surtout dans Kypriakai Spoudai 28 (1964) 8–12 (liste qui serait à compléter).

4 Pour les noms en Ἑλλο-, voir déjà F. Bechtel, Die historischen Personennamen des Griechischen. . . (ci-après HPN), 1917 (réimpr. 1982) 152, et mes remarques dans Etrennes de septantaine, Mélanges Michel Lejeune, Paris, 1978, 126–127.

5 L. Robert, Hellenica II, 1946, 75.

6 Cl. Rolley et O. Masson, BCH 95 (1971) 295–304; Inscr. chypr. syll., seconde édition, p. 422, no. 369a, fig. 154.

7 Strack, Antike Münzen Nordgriechenlands II.1, 1, 1912, 54, no. 44; J.F. May, The Coinage of Abdera, 1966, 138 sq., nos 184–185 (2 exemplaires).

8 Voir les listes abondantes chez R. Münsterberg, Die Beamtennamen auf den griech. Münzen, réimpr. 1973, 22–26, etc.

9 Dans Glotta 32 (1953) 219–222 (= Kl. Schriften, 1959, 241–243), Manu Leumann a étudié divers noms en -υς, comme Θράσυς, Ἄνδρυς, etc. Le premier correspond à l'adjectif θρασύς, d'autres sont clairement des diminutifs, cf. la liste de Bechtel, HPN, 52, au sujet de Ἄνδρυς.

10 Leumann, Homerische Wörter, 1950, 141 sqq.; Chantraine, Dict. étymol. de la langue grecque, s.v.

10a Je l'étudie dans Rev. Phil. 1986.

11 Déjà dans Athenian Agora XII, 1970, no. 627, Pl. 27; même texte, reproduit chez M.L. Lazzarini, Le formule delle dediche votive nella Grecia arcaica (Memorie Lincei, 1976), 224, no. 346 (non repris dans le SEG).

12 Bechtel, HPN, 348 sq. (l'existence de composés en -ονητος) n'est nullement assurée).

13 Rev. Phil. 1963, 220 (à propos de -μορτος et Μορτο-).

[14] Liste chez Bechtel, ibid. 156–159. La combinaison d'éléments qui sont employés dans l'onomastique d'une même famille permet d'expliquer la formation de noms nouveaux, d'aspect souvent "illogique" et irrationnel.

[15] Cf. L. Threatte, The Grammar of Attic Inscriptions, I, 1980, 136. On trouve cependant trois exemples d'"Ὄνασος dans la Prosopopographia Attica.

[16] Bechtel, HPN, 614; du même, Attische Frauennamen, 1902, 134.

[17] H. Solin, Die griech. Personennamen in Rom, Ein Namenbuch, 1982, 1224–1225.

[18] O. Masson, "Vocabulaire grec et anthroponymie. . .", Mélanges offerts à P. Chantraine, Paris, 1972, 119–122.

[19] Par exemple chez H. Wuthnow, Die semit. Menschennamen. . . , Leipzig, 1930, 70; pour l'Egypte, dans les recueils de Preisigke et Foraboschi.

[20] Wuthnow, ibid. Pour les noms MLK et MLK' à Palmyre, voir J.G. Stark, Personal Names in Palmyrene Inscriptions, Oxford, 1971, 95.

[21] On remarquera en passant qu'un féminin assez ancien Μαλίχα attesté en Attique, IG II^2, 9112, aussi 12028, représente un nom bien grec, Bechtel, HPN, 494, avec une interprétation assez plausible.

[22] Chez Bechtel, HPN, 339, un exemple de Clazomènes (IVe s.).

[23] Texte reproduit dans SEG XX, 735b I, l. 115 et 116 (nominatif et génitif).

[24] D.M. Robinson, AJA 51 (1947) 368, Pl. 89b-c; cf. J. et J. Robert, Bull. Epigr. 1949, no. 61; non repris dans le SEG.

[25] Article cité, avec renvoi à Bechtel, Att. Frauennamen, 7–8 (liste des composés normaux en *-αρίστη*).

[26] Voir L. Threatte, o.c. 121 et 391; Chantraine, Dict. étymol. s.v. *Ψάμαθος*.

[27] L. Threatte, o.c. 400, avec des parallèles.

[28] Bechtel, HPN, 592–595.

[29] Voir aussi M.J. Milne et D. von Bothmer, Hesperia, 22 (1953), 220, pp. 66b, avec SEG XIII, 32 (commentaire).

[30] Bechtel, HPN 543 et 546.

[31] L. Threatte, o.c. 486 (devant ***beta***), avec aussi cet exemple.

[32] Bechtel, HPN, 597; Att. Frauennamen, 106. Davantage de détails dans Spitznamen, 1898, 76.

[33] Mabel Lang corrige ainsi une lecture "*Σιβακώ*" donnée d'abord dans Hesperia Suppl. VIII (1949) 407.

[34] Je me suis occupé à plusieurs reprises des métronymes: voir en dernier lieu BCH (1983) 397.

[35] Hypothèse formulée par D.M. Lewis dans SEG XXIX, 287.

[36] Texte utilisé par Bechtel, HPN, 551 et 554; Att. Frauennamen, 60–62.

[37] Bechtel, HPN, 543, avec IG II^2, 1951, 23 (rectifier ainsi la référence dans Agora XXI, 34). L. Robert, CRAI 1968, 419, signale aussi un *Τριβαλλὸς χρηστός* dans IG II^2, 12822, ainsi qu'un homme libre, père d'un mercenaire à Abou Simbel, A. Bernand, O. Masson, REG 1957, 30, n. 22, à propos d'un *Τριβαλλός* militaire en Bactriane, Robert ibid. 418–420 (milieu du IIIe s.).

[38] Bechtel, HPN, 546.

[39] Voir sur ce point Threatte, o.c. 536.

[40] Threatte, o.c. 529 (type *Ἄριστος*, il n'y a pas lieu de supprimer le second sigma).

[41] Bechtel, HPN, 553.

[42] Ibid. 549.

[43] M. Guarducci, Inscr. Creticae I, 1935, 47, avec les auteurs concernés; aussi les articles correspondants du Pape-Benseler.

[44] On terminera par une note sur le graffite C 23, Pl. 5. Dans ce texte obscène (avec le composé aristophanesque *λακκόπρωκτος*), un nom "*Συδρόμαχος*" est non seulement "inconnu", mais impossible. Comme l'a remarqué D.M. Lewis, SEG XXIX, 287, il convient d'envisager un lapsus et de retrouver (*Κ*)*υδρόμαχος* (la main est très hésitante).

Some Remarks on Yasna 34

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1. Yasna 34.2c: It has the expression *pairigaēΘē xšmāvatō vahmē* which has been rendered by Humbach (1959) as 'bei der die Herden übermittelnden Verherrlichung von euresgleichen'. On the strength of certain parallel passages Humbach believes that *pairigaēΘa-* stands for something like **pairī.dā.gaēΘa-* or **pairī.dāta.gaēΘa-* meaning 'den lebenden Besitz übermittelnd'. Insler (1975), on the other hand, feels that *pairigaēΘa-* means 'universal'. He compares with this word the Vedic expression *párijman-* which, according to him, means 'around the earth, universal'. He therefore concludes that *pairigaēΘa-*, which literally means 'around the creatures', also means 'universal'. He translates the above expression as 'in universal glory (p. 55) or praise (p. 221) of your kind'.

The context in which the above expression occurs is of a glorification (*vahma-*), accompanied by praise-songs, performed by an individual, and yet, apparently, many persons are concerned in this act of glorification as is shown by the plural form *əhmā* 'by us' in the preceding stanza.[1] Hence it seems better to interpret *pairigaēΘa-* as 'surrounding the living persons in the household', i.e. (a glorification) in which all the members of the household participate.

2. Yasna 34.5ab: *kaṯ vā xšaΘrəm kā īštiš šyaoΘanāi mazdā yaΘā vā ahmī*[2], *ašā vohū manaŋhā Θrāyōidyāi drigūm yūšmākəm.* Insler's rendering runs as: "Have ye the mastery, have ye the power, Wise One, for the act to protect your needy dependent – as I indeed am – with truth and with good thinking? " He thus takes the parenthetic phrase *yaΘā vā ahmī* with reference to *drigūm yūšmākəm.* He connects *ašā vohū manaŋhā* with *Θrāyōidyāi*, for he does not approve of treating it as commitative instrumental with vocative *mazdā.*

It seems, however, preferable to relate the parenthetic phrase with those words between which the phrase stands, viz. *šyaoΘanāi* and *ašā vohū manaŋhā*, and not with those which stand away from it. This also means that one connects *ašā vohū manaŋhā* with *šyaoΘanāi.* What is meant is that Zarathustra wants to know what kind of might, what kind of power Ahura Mazdā has for (1) the performer who acts (lit. an act performed) in accordance with truth, and with good mind, as indeed, Zarathushtra himself is (*yaΘā vā ahmī*), i.e. a performer of a truthful act done with good mind, and (2) for protecting those who are dependent on Ahura Mazdā.

3. Yasna 34.9ab: *yōi spəntąm ārmaitīm . . . duš.šyaoΘanā avazazaṯ.* Humbach and Insler, following Bartholomae, treat *duš.šyaoΘanā* as an adjective and render it, respectively, as 'die Übeltäter' and 'those of evil actions' (nominative plural masculine). But it is better to

treat it as a noun 'evil action' and render the line as 'those who have abandoned virtuous piety by evil action'. This interpretation is recommended, on the one hand, by the close proximity of *duš.šyaoϴanā* and *avazazaṯ*, and, on the other, by the fact that *duš.šyaoϴanā* is thus contrasted with *vaŋhāuš manaŋhō šyaoϴanā* 'with (good) act stemming from good thinking' of stanza 10.

Since stanzas 9 and 10 are related in that the activities of the bad and the good are contrasted, it is possible to imagine *duš.xratu-*, as opposed to *huxratu-* (st. 10), as the subject of 9 ab: *yōi* (= *duš.xratavō*) *spəntąm ārmaitīm . . . duš.šyaoϴanā avazazaṯ.*

4. Y. 34.9c: *aēibyō maš ašā syazdaṯ.* Humbach treats *ašā* as nominative plural and translates, 'von denen sollen sich schnell die Wahrhaftigkeiten entfernen'. Insler, however, treats *ašā* as instrumental singular and, accordingly, translates the passage as 'from them one has certainly retreated in accordance with truth'. It is, however, worthwhile to consider if *ārmaiti* can be the subject: '(those of evil intentions who have abandoned piety), from them (piety) shall quickly retreat along with truth'. In stanza 10 piety is said to be *hiϴąm ašahyā* 'Genossin der Wahrhaftigkeit'[3], and hence it is reasonable to suppose that when she recedes from the perpetrators of evil actions, she does so along with truth. When evil-thinkers abandon piety, piety too, along with truth, leaves them.

5. Yasna 34.13ab: *tąm advānəm ahurā yąm mōi mraoš vaŋhāuš manaŋhō, daēnå̊ saošyantąm yā hū.kərətā ašāciṯ urvāxšaṯ* "die Bahn, o Lebensherr, die du mir als die des Guten Gedankens genannt hast, die gutgebahnte auf der die Sinne der Kraftspender mit Wahrhaftigkeit wandeln" (Humbach). Humbach thus treats *daenå̊* as the subject and *hū.kərətā* as referring to *advan.*

Insler, however, reads *yāhū karətā* and treats *karətā* as nominative singular masculine of *karətar-* 'extoller, commemorator' and *daēnå̊* as accusative plural feminine to which the relative *yāhū* refers. He translates: "To that, Lord, which Thou hast told me to be the road of good thinking, to the conceptions of those who shall save, along which Thy extroller shall proceed in alliance with truth indeed. . ."

With *karətar* 'extoller' Insler compares *carəkərəϴra-* 'hymn of praise' (Y. 29.8c). It seems possible to connect these two words etymologically with Vedic *kīrí-* (*kīrín-* ?)[4].

Notes

[1] Also cf. *dāmā* (plural) in stanza 3, and *gaēϴå̊ vīspå̊* in the same stanza which is nominative plural according to Insler.

[2] Humbach prefers to read *hahmī* 'wie wenn ich schlafe'.

[3] Insler, however, interprets this as referring to Ahura Madzā.

[4] On the doubtful nature of the stem *kīrín-*, cf. M.A. Mehendale, BSOAS 37.1974:670–671.

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Folk-linguistics and the Greek word

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1. Debts should be acknowledged even when they cannot be repaid. I have endless other obligations but there is no-one to whom I am more deeply indebted, both personally and intellectually, than Henry Hoenigswald. He has encouraged me when I hesitated, helped me when I was in trouble, corrected me when I was wrong, and above all rescued me from obscurity when I was confused. He has been friendly when we agreed, friendlier when we disagreed. This is not easy to express without pomposity and sentimentality – two sins of which Henry could never be guilty. I risk incurring his disapproval but, on this occasion at least, καλὸς ὁ κίνδυνος.

In 1966 Hoenigswald pointed out that if the model of social sciences is to be taken at all seriously linguists ought to be interested "not only in (*a*) what goes on (language), but also in (*b*) how people react to what goes on (. . .) and in (*c*) what people say goes on (talk concerning language)". He also warned that "it will not do to dismiss these secondary and tertiary modes of conduct merely as sources of error" (Hoenigswald 1966: 20). Folk-linguistics, the discipline whose study Hoenigswald advocated, is closely linked with ethnography of speech and ethnolinguistics but is more decidedly slanted towards a study of the speakers' reactions to language and of their talk about language. It is an obvious hypothesis that it has a direct and unique link with the history of linguistic thought or, as we more normally say, the history of linguistics. In what follows I want to start from a specific point made by Hoenigswald in his 1966 paper and examine it in some detail with reference to Greek and to the beginnings of Greek linguistics.

2. Hoenigswald (1966: 18) considered the advantages of a more technical investigation for those cultures in which we observe the existence of "a formalized activity in which language is the object of reverence, contemplation, or study", and noted that the first approach to the subject "is necessarily by way of vocabulary"; we might start by asking, for instance, for the equivalents of items like 'word' or 'sentence' and inquire, inter alia, whether they exist and are used in a definable sense. In this context he referred to "the old observation that the Greeks had no word for 'word'", but since his purpose was of a more general nature, the observation remains isolated in the article. In my turn I want to ask what notion, if any, the Greeks had of 'word'. My question is not of a linguistic nature (what was a Greek word? how do we define a Greek word?); it is rather a metalinguistic question, if I can use 'metalinguistic' in this extended sense. Yet, it is not, or not predominantly, a question in the history of linguistics; it is mainly a question in Greek folk-linguistics.

Why is the question worth asking and answering? First, because an answer will form an integral part of an account of the Greek secondary responses to language. Secondly, because, if the answer is based on evidence which is not entirely lexical, it may lead to some more general observations about the parallelism – or lack of it – between metalinguistic lexicon and metalinguistic reactions. Thirdly, because it is at least possible that the notion of word belongs both to folk-linguistics and to linguistic analysis proprie dicta. If so, an answer – or the work leading to it – may serve to test the existence and nature of the link and/or continuity which I hypothesized earlier between folk-linguistics and linguistics. If this link exists it is in Greece that it ought to be most apparent. The whole tradition of linguistic thought in the West at least as far as the nineteenth century is directly or indirectly based on Greek thought; no doubt at each stage cultural influences of varying types were also important but they did not destroy the basic continuity. From a Western point of view it is in Greece that scholarly awareness of language starts; if so, it is in Greece that we ought to test the connection between folk-linguistics and scholarly work. There are other ways in which one could justify both the question and the time and labour spent in answering it; for the moment I see no advantage in going into greater detail.

3. What evidence do we have at our disposal? Obviously we must start with the lexicon, but two other types of evidence spring to mind. First, writing and all possible forms of script; secondly, explicit statements which we may be lucky enough to have from any of the preserved texts.

The lexicon has been explored already and Hoenigswald's statement quoted earlier is frequently found in the secondary literature and is basically correct (Fournier 1946: 225; Allen 1974: 113, note 1). In Homer ἔπος and μῦθος are frequently translated with 'word(s)', but both terms would be more correctly rendered with 'speech', though μῦθος at least may also refer to thought or story. We have no evidence that Homer had the lexical resources necessary to ask questions such as 'is X one or two words?' or 'are these two words of the same type?'[1]. In Herodotus we find that ἔπος can refer to one word (βέκος in the Psammeticus story of 2,2, the name Ἀσμάχ in 2,30), but also means proverb (e.g. τὸ παλαιὸν ἔπος in 7.51) or verse (e.g. Ὁμήρου ἔπος in 4,29) and can be used to refer to a sentence or a phrase (e.g. ἐνὶ δὲ ἔπει . . . in 3,82). Of the other possible terms, μῦθος is very rare and means 'story, myth'; ῥῆμα is not frequent, occurs mostly in the plural and is best translated with 'saying(s)' or 'speech'; λόγος has its usual wide range of meanings. In Attic the closest equivalents to 'word' are λόγος, ῥῆμα, and ὄνομα. It is hardly necessary to reiterate that λόγος (which eventually became the technical term for 'sentence') means far more than 'word'[2]; ῥῆμα is not much better defined. In Plato it refers to a proverb or saying (e.g. *Prot.* 343 b), to a phrase or expression such as Διὶ φίλος (*Crat.* 399 a), to a single word (*Tim.* 49 e), to a predicate or verb (*Soph.* 262 a). The modern translator is constantly in doubt: should the συνηθείας ῥημάτων τε καὶ ὀνομάτων mentioned in *Theaet.* 168 b be rendered as "sens coutumier des expressions et des mots" (Diès 1955: 196), "habitual use of expressions and words" (McDowell 1973: 42), or – less likely – does it refer to verbs and nouns as implied by LSJ s.v. ὄνομα (vi.2)? Similar problems arise for ὄνομα. In Homer it refers only to 'fame' or to 'proper name'. In Attic it may mean 'word', but also 'name' and, later, 'noun' as contrasted with ῥῆμα 'verb' or 'predicate'. In the famous passage from the Cratylos mentioned earlier (399 a) does Plato speak of the shift from the phrase Διὶ φίλος to the name Διόφιλος or of the shift from a phrase to a single word?[3]

Later on further terminology – which is now technical terminology – comes in. Dionysius of Halicarnassus (First Century B.C.) uses ὄνομα for word in his earlier works but in his later contributions speaks of ὀνόματα or λέξεις or μόρια λόγου (Schenkenveld 1983: 70). Householder (1981: 4) points out that Apollonius Dyscolus (Second Century A.D.) uses three equivalents for 'word': μόριον, λέξις, and μέρος λόγου or μέρος τοῦ λόγου; ὄνομα, however, still appears as a cover-all term (Ap. Dysc. *de synt.* 18, 2). We may discuss the exact value of these forms but there is little doubt that by this stage the one-or-two-words and the word-type questions I hypothesized earlier can be asked and answered. I shall return to this later.

What emerges from this brief survey is not, as has been stated, that the notion of word appears late in the development of Greek thought, but rather that the Greek lexicon does not offer any clear evidence for the existence of such a notion; yet, it does not offer any evidence for its absence either. Even when technical thinking about grammar starts and a notion of word is obviously available, the vocabulary does not make this immediately clear.

4. The easiest way of defining a word in English is with reference to the written language: a word is a sequence of letters written between empty spaces and/or some form of punctuation. This statement is obviously trivial and, in any case, can only refer to word-forms rather than, say, lexemes, but may well account for the confidence with which the layman speaks of words, in spite of the notorious difficulties encountered by linguists in their attempt to produce a definition of this concept.[4] A parallel statement is impossible for classical Greek. In the absence of spacing, accents, and smooth breathings, there was no formal device used to distinguish words in the written language. Admittedly one letter (*h*) in some regions at least occurred only in word-initial position and some letters could not occur in word-final position, but here writing reflects speech. We do not know that these facts were the object of metalinguistic observation and consequently we do not know whether they are relevant to our inquiry. If so, do we learn anything from the written language?

The answer is definitely positive if we take into account the whole gamut of writing which we find in ancient Greece. Of the three scripts used in Greece for Greek (Linear B, syllabic Cyprian, and the alphabet) it is only the third, and even this in a limited part of its history, which seems to provide no evidence for the Greek notion of word. The first two definitely repay further analysis and the alphabet itself is not as barren of information as at first appears to be.

The data vary in time and space. Linear B was written in the second half of the Second Millennium B.C. in Crete, in the Peloponnese, at Thebes, and possibly in other parts of Mycenaean Greece. Syllabic Cyprian, with the exception of one early document, dates from the first part of the first Millennium B.C. and stretches until the Hellenistic period; most of the texts come from Cyprus.[5] I do not need to rehearse here the basic data about the Greek alphabet. The three scripts are independent of each other. There is genetic affinity between Cyprian and Linear B, but we do not know the exact nature of the relationship. There is a chronological overlap between Cyprian and the alphabet but until the latest period at least no obvious influences of one on the other.

If what we aim at is a view of what the ancient Greeks in general or, at any rate, the man in the ancient Greek street felt about language, we ought to use the evidence of writing with

extreme caution. The level of literacy in Greece was low at all times and for the potential users of Linear B and syllabic Cyprian must have been very low. It is only in the fifth century and only in some parts of Greece that writing acquired a different status and importance. Yet all that we can hope to reconstruct is the metalinguistic assumptions and beliefs of a small subset of Greeks; if so, we should not be deterred by the nature of our evidence. That the number of literate speakers was not unbearably small even in the earlier period is shown by the Linear B texts of Knossos. It seems probable that these all belonged to the same year and were written by a minimum of one hundred different scribal hands. The odds are that the scribes were not a caste separated from the others; there must have been a number of administrators who could write but had other tasks as well (Olivier 1977a; 1977b).

The syllabic scripts are often compared unfavourably with the alphabet. The latter is praised for its quasi-phonemic character, its simplicity and clarity – qualities which are missing in the former. Linear B and, to a lesser extent, Cyprian are reproached for the ambiguity of their graphic systems and their incapacity to express all grammatical distinctions. Yet in our strictures we ignore a basic point: the syllabic scripts provide information which the alphabet does not offer. From the alphabet, for instance, we learn very little, if anything, about syllabic division; from the syllabic scripts we learn a great deal. For a question which is closely related to that from which we started – that of word division, i.e. written identification of word-forms – the syllabaries are highly informative and far more obviously so than the alphabet.

5. Let us start from Linear B. The script has a sign used as a word-divider which has no exact match in any standardized form of the Greek alphabet until spacing is introduced in the Middle Ages. The regular use of the word-divider is an innovation of Linear B; the sign is not found – or found only sporadically -- in the earlier Cretan scripts to which Linear B is related.

In Linear B the word-divider is normally a small vertical bar (the size varies) and is used with some regularity between sequences of syllabograms to separate them one from the other. It does not occur at the beginning of a text nor does it normally occur at the beginning or end of a line – which effectively prevents us from speaking of a preposed or postponed 'word-determinative'. The word-divider can also – but need not – be omitted

(a) when it is replaced by an empty space,
(b) when the size of the characters changes (e.g. from large to small), between a word and the following one,
(c) when the word is followed by an ideogram or number.

For the rest in the vast majority of the cases we find a word-divider where a modern editor would leave spacing between words. Yet there are exceptions which can be subsumed under a few main types and ought to be considered.

First, the word-divider may be occasionally and unpredictably omitted, though none of the points made above apply. The omission seems accidental because the same sequence is written by the same scribe with and without word-divider (here conventionally indicated with a comma); cf. KN Fp 1, 10 (hand 138) *a-ne-mo, i-je-re-ja* vs. Fp 13,3 (hand 138) *a-ne-mo-i-je-re-ja* (twice); PY Fn 187,8 (hand 2) *u-po-jo-po-ti-ni-ja* (the editors doubt the existence of a word-divider) vs. Fr 1236 (hand 2) *u-po-jo, po-ti-ni-ja* (cf. also Fr 1225,1); PY Aq 64,2,4,14,15 *to-to, we-to* vs. ibid. 6,7,16 *to-to-we-to.*

Secondly, Linear B apparently avoids marking with the word-divider graphic words which would consist of one sign only; hence the spelling *to-so-pa*: *tos(s)os pas* in PY Ja749 and Jn601,9 contrasted e.g. with the plural *to-so, pa-te* (KN B 1055,9). An isolated sign could be taken for an ideogram or an abbreviation, so that presumably there is a graphic motivation for this rule.

A third set of exceptions could conceivably belong to our second category but is best treated separately. This consists of a number of particles, pronouns, etc. which are regularly not divided from the word which precedes or follows. Thus *-de* and *-qe* (Gr. δέ, τε) are always joined to the preceding word (and followed by the word-divider), *jo-* and *o-* (Gr. ὡς ???) are joined to the word which follows. Not all these spellings can be explained merely on the basis of the Mycenaean reluctance to write one-sign words separately since we find two-sign sequences treated in the same manner: *da-mo-de-mi* : *dāmos de min* (PY Ep 704,5), *e-ke-de-mi* : *ekhei de min* (PY Na 926), probably *o-da-a*$_2$ and *o-de-qa-a*$_2$; the negative particle *o-u-* (Gr. οὐ) is always joined to the word which follows : *o-u-di-do-si, o-u-di-do-to, o-u-pa-ro-ke-ne-*[, *o-u-te-mi, o-u-wo-ze* (*o-wo-ze*)[6]. The form *o-u-ki* (Gr. οὐχί) occurs only once and is joined to *-te-mi* which follows (KN V 280,5; cf. *o-u-te-mi* ibid., 11–14). By contrast *o-u-qe* (Gr. οὔτε) is frequently found at Knossos and at Pylos as an independent word, preceded and followed by the word-divider. No doubt we deal here with elements comparable with the prepositive and postpositive elements of the metricians (Maas 1962: 84ff.; West 1982: 25f) but the problem is how our particles are to be defined. I wonder whether a merely distributional and syntactical account is sufficient. Mycenaean has a number of disyllabic prepositions (*o-pi, e-pi, pa-ro* etc.); in the noun phrases in which they occur they are normally (not always) separated from the noun which follows by a word-divider. Distributionally everything points to a 'prepositive' status of the preposition; yet the 'prepositive' *o-u* 'not' is regularly joined to the following word; the prepositions are not. The different spelling may be explained if *pa-ro* was orthotonic, but *o-u* was not. For later Greek it is often suggested that the prepositions were proclitic; in Mycenaean the position must have been different, but the suggestion agrees with all that we know about the historical development of the prepositions, which certainly started as orthotonic and presumably nominal elements.[7]

A preliminary conclusion at this stage is that in Mycenaean the word-divider is used to separate accentual groups, i.e. speech sequences characterized by one main accent; the exceptions are explained by graphic reasons. However, before this can be accepted, we must consider two further sets of exceptions.

I have mentioned above the odd instances of omission of word-divider due presumably to negligence or sometimes to lack of space. There are also two instances of sequences which ought to be written with a word-divider but occur consistently and more than once without. In Knossos *pa-si-te-o-i* : *pansi theoihi* 'to all gods' occurs 15 different times in tablets written by four different scribes (Fp-, Ga-, Gg- passim); in Pylos *we-te-i-we-te-i* 'every year' (Fετει-Fετει) occurs 13 times in the same tablet (Es644) again without word-divider. In the second sequence we may well have an instance of univerbation. The formation is archaic[8], we have no other examples of it in Mycenaean, and it may have survived in a fossilized and 'univerbated' status with consequent accent simplification.[9] The case of *pa-si-te-o-i* is more puzzling. Semantically, the odds are that 'All gods' count as a unit, a single denomination; yet, we expect that if we had more texts we would also find a nominative **pa-te*(-)*te-o* : *pantes theoi* and a genitive **pa-to te-o* : *pantōn theōn* with both elements declined, so that

on grammatical grounds it would be difficult to attribute word status to the phrase. The position is only partially comparable to that of the Homeric κάρη κομόωντες for which even οἱ παλαιοί, according to Eust. 165, 14f., disputed whether it was one or two words (cf. also *Schol. in Hom. Il.* B 11 b Erbse). In the absence of further evidence we must conclude with a non liquet, though I doubt that this is sufficient counterevidence to destroy the suggestion made earlier.

Another difficulty concerns the odd instances of compounds in which the first element is separated from the second by a word-divider. These are constantly quoted in the literature (most recently Risch 1983: 377f.), but no attempt is normally made to assess the extent and the implications of the phenomenon. In fact, in constrast with the very large number of compounds attested in the Linear B tablets and written as a single word, there are only a minimum of three and a maximum of five compounds with this abnormal spelling.[10] They are all attested in a single set of tablets, the Ta tablets of Pylos: *ke-re-si-jo, we-ke* : *Krēsio-wergēs* 'of Cretan workmanship' (Ta641,1.1; Ta 709, 3.3); *e-ne-wo, pe-za* : *en(n)ewopeza* 'nine-footed' (Ta 642, 1.3; contrast *e-ne-wo-pe-za* Ta 713, 1.3; Ta 715, 1); *pu-ko-so, e-ke-e* : *pukso-(h)ekhehe* 'with box-wood support' (?) (Ta 715,3); *a-pu, ke-ka-u-me-no* [: *apukekaumenos* 'burnt off' (Ta 641, 1); *a-pi, to-ni-jo* ??? (Ta 716, 1). The first three forms must be compounds because the morphology of the second element would otherwise be unexplainable. The position of the last two words is more difficult to understand: *a-pi to-ni-jo* is obscure and though it could be a compound there is no certainty about it. In *a-pu ke-ka-u-me-no* there is just a chance that *a-pu* is adverbial, though otherwise tmesis is not found in Mycenaean. It would be rash to base any conclusion on these two forms, but the first three compounds are disturbing: if the word-divider is used to mark separate accentual units, and if these words are compounds, i.e. – in Greek terms – accentual units, why are their elements separated by the word-divider? Two suggestions are possible. First, we could argue that besides the accentual and other criteria which justify the treatment of compounds as single words, the Mycenaean scribes consciously or unconsciously made use of semantic criteria which induced them to treat compounds as formed of separate words. The problem is not normally discussed but I suspect this is the view which would be generally accepted. Yet, it does not explain why the divided compounds are so limited in number and limited to one set of tablets. A second possibility, and one which I favour, is that the scribe of the Ta tablets produced new words while he was writing ('nine-footed', 'made-in-Crete', 'with-box-wood-support') and that he marked off the separate elements in order to facilitate comprehension – much in the same way in which British English uses hyphenated forms ('tax-free') or open forms ('reading material') to indicate newly formed compounds which have not yet gained permanent status and consequently are not written as solid words ('bedroom').[11]

For Mycenaean the conclusion must be that the use of the word-divider is mainly determined by accentual criteria, though conceivably other criteria may play some part. We may feel entitled to argue that the Mycenaean scribes had a notion of word (word-form) consciously or subconsciously defined in accentual terms; from an English point of view this would mean that 'blackbird', 'cat', 'the cat', and 'kill him' are all treated as one-word sequences. On the other hand it would be wrong to generalize from here and argue that the Mycenean scribes analysed the spoken chain in predominantly phonological terms. It seems likely, and a few pieces of evidence confirm it, that the syllabification pattern in most cases ignored word-boundaries; thus if the syllabification was expressed in writing in the usual

manner for a Mycenaean phrase [ona:ton (h)ekhei] we would expect a Linear B spelling **o-na-to-ne-ke*. Yet what is really attested is *o-na-to, e-ke* (PY En-, Ep-passim) or even *o-na-to-e-ke* with omitted word-divider *and* omission in writing of *-n* in word final position (PY Ep 539, 3; 613,7): correct word division is more important than correct syllabification.[12]

6. Such much about Mycenaean. What about the other scripts? In the Cyprian syllabary word-dividers (bars, dots, empty spaces) exist (Masson 1972: 107ff; 1983: 68ff.) but are not used with as much regularity as in Mycenaean and sometimes mark phrases rather than individual words. Yet, word division is more reliably indicated by another device. In Linear B the consonants which close a word are normally not written; in Cyprian the final consonant of a word is written with a sign of the *Ce* type: cf. *pa-si-le-u-se* : βασιλευς; *po-to-li-ne* : πτολω. In internal position different rules apply: cf. *ka-ra-si-ti* : γρασθι; *pa-ta* : παντα. It is noticeable that here too word-division takes priority over syllabification: in the sequence πτολω Ἐδαλιον the final *-n* of πτολω must have formed a syllable with the first vowel of Ἐδαλιον but the script renders it as if it belonged to πτολω only: *po-to-li-ne-e-ta-li-o-ne*, not **po-to-li-ne-ta-li-o-ne*.

If we can judge from the spelling, monosyllabic enclitic and proclitic elements are not treated as independent words[13]: cf. *a-u-ta-ra-mi* (not **a-u-ta-re mi*) for αὐταρ μι (με); *ta-sa-ke* (not *(ta-se ke)* for τας κε. Other enclitic particles are παι, νυ etc. Some proclitics are easily identified: cf. *to-no-ro-ko-ne: τον ὀρκον* (not **to-ne o-ro-ko-ne*); *su-tu-ka-i*: συν τυχαι (not **su-ne tu-ka-i*). Yet, as Hermann (1906: 240ff.) saw a long time ago, the distinction here is between particles which end in a sibilant and are written as independent words, and particles which end in a nasal and are joined to the word which follows; contrast *ta-se a-ta-na-se* : τας Ἀθανας (Masson 1983: 217, 20) and *to-i-se ka-si-ke-ne-to-i-se* : τοις κασιγνητοις (ibid., 5) with *ta-po-to-li-ne* : ταν πτολω (ibid., 1) and *ta-na-ta-na-ne* : ταν Ἀθαναν (ibid., 27). In spite of the different renderings, Hermann assumed that all these elements (particles, articles etc.) were proclitic, and explained spellings such as *ta-se* : τας, *po-se* : πος, *ka-se* : κας 'and' as due to some form of hypercorrection or linguistic conservatism. Intervocalic *-s-*, he agreed, had changed into *-h-*, a sound impossible to indicate with the normal graphic resources of the syllabary; the spelling of the *ta-se* type was meant to support the etymologically correct pronunciation with *-s*. The speakers had no difficulty in uttering a sibilant in word-final position or before pause and consequently that type of spelling was adopted. The texts published after Hermann's article have not changed the position. His explanation may be correct and is accepted by Masson (1983: 70) but a difficulty remains. For e.g. κας we have before a word which began with a vowel two possible spellings from a relatively early date: either the *ka-se o-na-si-lo-i* : κας Ὀνασιλοῖ type or the *ka-a-ti* : κα(ς) αντι type. The latter example shows indeed that *-s* either dropped or became *-h* thus supporting Hermann, but Hermann has not succeeded in explaining why the third possible (and expected) spelling **ka-so-na-si-lo-i* is normally not attested[14], though all early inscriptions preserve intervocalic *-s-* (in writing at least). As an alternative one might suggest that in word-final position *-s* tended to become *-h* and consequently the *-se-* sign was felt to have both an *-s-* and an *-h* value, differently from e.g. *-sa-*, which was not used to express final *-s*. An article like τας may have been pronounced [tah] and this may have called for one of two spellings, either *ta-se* or *ta*. If so, the Cyprian writers may have sacrificed the correct rendering of syllabic and word junction to a correct rendering of the consonantal

nature. This explanation too cannot be demonstrated, and the problem remains open, but we must agree with Hermann that the odd spellings of the *ta-se, ka-se* type must be due in some way to the presence of the *s*; they do not prove that these words were not proclitic or were not felt as such.

I now return to the original question: what categories did the Cyprians mark in writing? The use of the word-divider is too irregular to be of much use; the use of the *-Ce* convention is more informative. It shows that a 'word' was either an orthotonic word or a sequence of orthotonic and unaccented elements.[15] The unexpected spelling of e.g. *ταϛ* as contrasted with e.g. *ταν* may imply that a new concept of word was evolving (perhaps under the pressure of phonological facts) and that the proclitics were attributed a more independent status than the enclitics.[16] The evidence is not compelling and it may be safer to retain from this discussion one conclusion only: accentual factors played a considerable part in determining the notion of word which was influential in Cyprian spelling.

7. Cyprian is important in that it may provide us with the link we need between the Linear B and the alphabetic texts. Mycenaean writing, as we have seen, reveals an implicit notion of word mostly based on accentual criteria; we ought to ask whether this notion survived the collapse of Mycenaean civilization. Cyprian shows that, admittedly in a relatively isolated area, this happened. Should we now assume that those Greeks who wrote in the alphabetic script well after the Mycenaean period but contemporaneously with most of the Cyprian texts shared this notion?

In classical Greece most inscriptions do not mark word division and smooth breathings and accents are not written. Yet some data are available. Some texts avoid breaking words at the end of a line. Others use bars or dots or other devices as a form of interpunction within the sentence or paragraph. This happens in a few regions only and mostly in the sixth and fifth centuries B.C., though earlier and later examples are found; within this limited period we can gain some information.[17]

With the notable exception of Threatte's (1980: 73ff.) detailed discussion of Attic data until the third century A.D., there is no modern study of the punctuated texts, and most editions of these inscriptions are unreliable in their rendering of punctuation.[18] What follows is simply a first attempt at making use of a material not sufficiently analysed.

We cannot assign a single purpose to punctuation. We find it at the end of a paragraph or a line or a verse; it may also be used to mark the first word of a paragraph or an important name or number; sometimes it may even serve as a decorative element. In some texts it seems to divide words, in others phrases and/or words. The extraordinarily rare cases where it occurs in the middle of a word must be due to a mistake[19]; we could go further and say that one of the important points is that punctuation does not occur in the middle of a compound or a word. In a few texts punctuation is clearly connected with word division in a predictable manner. Dots, bars etc. correspond to a high degree with the spacing introduced by the modern editor, but in addition to a few unpredictable divergences there are some regular disagreements. We may use as evidence two sets of examples: first, the Teiae Dirae, a fifth century imprecation from Teos written on two stelae, of which one is known only from old drawings and editions (the stone was lost) and one has been recently found[20]; secondly some archaic texts from Argolis: a representative sample is provided by L.H. Jeffery (1961: 151ff., plates 26–31). Finally, a countercheck is possible thanks to Threatte's analysis of the archaic texts from Athens.

The Teiae Dirae make regular use of the colon, two dots one above the other. This is omitted at the end of the line, but otherwise separates words. A more complete statement would further add that:

1. no form of the article is separated from what follows (*τό, τόμ, τήν, τō, τά, τός, τάς*) and the prepositions are treated in a similar manner (*ἐκ, ἐν, ἐπί, ἐς, κατά, περί, πρός, ὑπό*(?));

2. the particles *ἤ* 'or' and *καί* 'and' are not followed by the colon and the same applies to the negatives *οὐ, οὐδέ, μή*;

3. two of the previously listed elements may follow each other (conjunction + article, preposition + article) without any intermediary punctuation or punctuation after the second of them;

4. two postpositives (*δέ* and *ἄν*) are attested and again they are not preceded by punctuation.[21]

The archaic inscriptions of Argos and Mycenae vary in their use of punctuation (normally dots). Some do not have it at all, one (Jeffery 1961: 169 no. 20) has a series of oddities (punctuation after *τᾱς*, whole phrases marked by punctuation), others (Jeffery 1961: 168 nos. 6, 8, 9, p. 169 nos. 19, 22, 26, 28, p. 170, no. 32; p. 174, no. 2) divide words according to criteria very similar to those found in the Teiae Dirae: the article in its various case forms and the prepositions are not followed by punctuation nor are *αἰ* 'if', *καί* 'and', and *μή* 'not'; the postpositives *δέ* and *τε* are not preceded by punctuation.[22]

For Attic Threatte (1980: 79ff.) has listed the main categories to be considered. Interpuncts characterize private inscriptions of the sixth and fifth centuries but are rarer in public texts of the fifth century, where they also have more specialized uses; in the texts where the main purpose is that of dividing words the usual rules apply: prepositions, *καί*, and the forms of the article are not separated from the word which follows.[23]

Thus the general pattern of written 'word-division' is relatively consistent; barring mistakes and oddities, a 'word' is either an orthotonic word or a sequence of proclitics – orthotonic word – enclitics. Expertise in word-division, thus defined, was obviously more necessary for the Mycenaean or Cyprian scribes than for their alphabetic counterparts, but it is remarkable that all three shared similar conceptions of the 'word'. Continuity of school between Mycenaean and alphabetic writing is probably to be excluded; if so, we ought to reach the conclusion that the Mycenaean, Cyprian and alphabetic writers based their principles of word-division on a common response to speech which consciously or more probably unconsciously, analysed it on the basis of accentual criteria. The continuity between the three traditions is also important for another reason: it allows us to say that for the literate part of the population a notion of word, or, more exactly, of word-form, was available at a much earlier stage than the terminology might induce us to believe. We are now in a position to argue on the basis of some concrete evidence against the assumption that the metalinguistic lexicon provides a full account of metalinguistic responses.[24]

8. In addition to lexicon and writing I mentioned above a third type of evidence, that provided by the explicit statements of writers, thinkers and scholars. Here we are on the borderline between folk-linguistics and linguistic analysis; a distinction cannot always be made but a brief inquiry may be relevant to the problem I raised earlier about the continuity or otherwise between these two forms of response to language. What follows is by necessity allusive, incomplete and superficial. A fuller statement would call for a complete

account of what we know and do not know about the development of philosophical and linguistic thought in Greece.

A first observation is obvious. The recognition of 'eight parts of speech' which is characteristic of traditional grammar presuppose some notion of word or, more exactly, lexeme, since in effect the 'parts of speech' are word-classes. The standard histories of linguistics connect the eight parts of speech with the Alexandrinian grammarians and regularly refer to Dionysius Thrax. It is not surprising to see that Dionysius' Techne also offers a definition of word (λέξις) as the smallest syntactical element of sentence structure (λόγος).[25] If so, and in spite of the justified complaints by the scholia about the vagueness of Dionysius' definition, it is reasonable to assume that an explicit notion of word was available to Dionysius and his contemporaries – and probably to his predecessors. Yet, the increasing doubts about the authenticity of the Techne[26] make it difficult to base any chronological statement on this evidence so that we must turn to other sources. The definition of λέξις as φωνὴ ἐγγράμματος which Diogenes Laertius (VII 56) attributes to Diogenes of Babylon is ambiguous as to the meaning of λέξις (word or speech with emphasis on sound rather than sense?).[27] I shall discuss later better attested grammarians like Apollonius Dyscolus.

What about the earlier period? Here the evidence is difficult to assess and I can only mention a few relevant facts. The study of Homeric or poetic words must have started before the fifth century. Aristophanes (fr. 222 Koch) refers to them with the term γλῶτται which becomes or had become a technical term for rare or special words (cf. Arist. *Rhet.* 1410 b, *Poet.* 1457 b, see Cassio 1977: 75ff; for Greek Latte, 1925: 136ff; cf. also Pfeiffer 1968: 12, 41, 78f).

It may be argued that this scholarly interest in some types of words belongs more to linguistics than to folk-linguistics, but the interest in etymologies (word-etymologies), which is even earlier, straddles between the two fields. Later on, Prodicus' concern with synonyms and the discussion about ὀρθότης ὀνομάτων reflected in Plato's Cratylus (to give just a few instances) show an interest in language which focuses on words, their semantic analysis and their etymology. Yet, this is more relevant to a study of the general attitudes to language than of attitudes to words as such. The literary sources do not tell us what counted as a word, with what criteria, if any, speech was segmented, and whether the distinction between word-form and lexeme was at all clear; nor do we know at what stage questions of 'wordhood' were first raised. For Aristotle (and for Plato??) ὄνομα and ῥῆμα, 'noun' and 'verb', may be word-classes (lexeme classes), but in chapter 20 of the Poetics (if authentic) Aristotle lists under the heading of μέρη τῆς λέξεως 'parts of speech' a mixture of word-classes (noun, verb, etc.) and other units below and above the level of the word (στοιχεῖον, syllable, sentence etc.). This makes it unlikely that at this stage the notion of part of speech presupposes the notion of word, as it does in the later grammarians and probably in the Stoics. On the other hand the introduction of the notion of πτῶσις – however ill defined or ill understood – implies that in some instances Aristotle distinguished word-forms from lexemes.[28] Later grammarians will make this explicit distinguishing between the parts of speech (word-classes) and their παρεπόμενα/*accidentia* (inflection etc.). A concern with the category 'word' also appears in Aristotle's statements about compounds, which he clearly takes as single words. Just as Plato had contrasted the ῥῆμα 'Διὶ φίλος' 'dear to Zeus' with the ὄνομα ('name' or 'word') 'Διφίλος' (*Crat.* 399 a), Aristotle (*de int.* 16 a) contrasts the λόγος 'καλὸς ἵππος' 'beautiful horse' with the ὄνομα 'Καλλιππος' and

enunciates a rule which he then generalizes. The *-ιππος* element of Κάλλιππος has no meaning of its own; the individual elements of compounds differ from those of simplicia in that they have some force, but nevertheless are not significant in isolation (cf. also *poet.* 1457 a and see Ackrill 1963: 115ff.; Belardi 1976: 117ff.). In these statements there is a fair amount of unclarity; nor is it possible to see how far the formal divergencies between the compounds and the phrases with which they are contrasted matter and how far the definition is purely semantic. However, there is little doubt that the question of 'wordhood' has been raised.

So far we are still remote from seeing any link between the evidence provided by the various forms of writing we have examined and that provided by the literary sources. On the one hand we found some techniques for segmentation which seemed to presuppose a notion of word largely based on accentual criteria; on the other hand we have frequent citations of words, semantic and etymological discussions, etc., but we hear little about accentual criteria. Admittedly, if the text is not too corrupt, Aristotle is likely to have mentioned the *σύνδεσμοι* among his parts of speech (*poet.* 1457 a) and these certainly included enclitic forms. Yet since, as we have seen, not all Aristotelian parts of speech are word-classes this need not be significant; in other words, we do not need to assume that for Aristotle an unaccented element could be a word. The later grammarians will also list *σύνδεσμοι* and *ἄρθρα* among their parts of speech (word-classes) and there the implication must definitely be that some unaccented forms counted as words, in contradiction with the notion of word which we extrapolated from a study of writing. Yet before reaching the later stage we find sufficient evidence to prove that the accentual notion of word had some influence on Greek thought. We can look again at the famous passage from Plato's Cratylos which I have quoted earlier (399 a); we are told that in order to make of the *ῥῆμα Διὶ φίλος* an *ὄνομα* we must remove the second iota and pronounce the middle syllable grave rather than acute (*Διὶ φίλος* → *Διόφιλος*). This does not certainly amount to an accentual definition of word but shows conscious knowledge of a link between accentual factors and univerbation. In a passage of the Sophist (237 c) Plato refers to *τοὔνομα ... τοῦτο, τὸ μὴ ὄν* 'this name, not-being'.[29] The use of *ὄνομα* to refer to two words would be exceptional; it seems easier to assume that for Plato *μὴ ὄν* was a single word. A similar point, again in connection with a negative particle linked to the following word, may be made à propos of Aristotle's discussion in *de int.* 16 a, b of phrases like *οὐκ ἄνθρωπος, οὐχ ὑγιάνει*. These, we are told, are not nouns (*ὄνομα*) or verbs (*ῥῆμα*), nor is there a special name with which to call them; hence they can be called *ὄνομα ἀόριστον* or *ῥῆμα ἀόριστον*. I do not want to discuss the philosophical or logical implications of this view,[30] but it seems plausible that Aristotle would not have found it necessary to discuss the point or at least to discuss it in the way he does, if he had not conceived of *οὐκ ἄνθρωπος* and *οὐχ ὑγιάνει* as single words.[31] Here too the suggestion is that the proclitic negative is treated as part of the word which follows.

Finally I turn very briefly to some later and more detailed discussions of words which we find in the grammarians. I shall consider two examples only, both from Apollonius Dyscolus' Syntax. In III 123ff. (262 b) Apollonius begins to discuss the subjunctive and in 132 he tackles the problem raised by the conjunctions which normally precede this mood. He wants to argue that e.g. *δράμῃς* can occur by itself and not only as part of e.g. *ἐὰν δράμῃς*. Yet he is not too certain of his ground and the discussion is protracted (ibid. 133): "But if this needs further proof, we will add a little more to show that conjunctions are not

compounded with verbs into single words (εἰς ἓν μέρος λόγου). [If they were] how could other words intervene? E.g. ἐὰν σήμερον καὶ αὔριον ἀκούσῃς . . .". (Translation by Householder 1981: 203). Here Apollonius appeals, in the first instance at least, to the principle which Bloomfield much later will call the principle of indivisibility, but he is stopped in full flight by an (anachronistic) objection for which he himself is responsible. In Homer, we are told, an expected participle κατεδηδώς can be replaced by κατὰ ... ἐδηδώς with another word intervening between the preposition and the verbal stem. Yet we still want to argue that the form is a compound; if so, we must admit that divisibility is possible in compounds though they are single words. Does it follow that ἐὰν δράμῃς is a compound? Apollonius now invokes another principle (also used by Bloomfield in his discussion of words), that of structural regularity. The phrase ἐὰν δράμῃς has the same structure as phrases like ἵνα δράμῃς or ὅρρα πεποίθῃς, but here the two conjunctions ἵνα and ὅρρα have accents of their own, so that they cannot be parts of a compound. Accordingly, ἐὰν δράμῃς must also be treated as a phrase and not as a compound.

There is a third principle here and must be treated as conclusive: two main accents cannot belong to the same word. The progression of the argument seems clear. Starting point is the assumption that ἐάν belongs with the following word, an assumption which, I would argue, was part of the general belief in the earlier period. This is rejected for the reasons indicated. Apollonius seems to have found arguments to support his conclusion: while two main accents must belong to two words, one main accent is not the mark of a single word. Proclitics and enclitics are unaccented and yet they can count as independent words.[32]

A question which concerns the nature of words is discussed later in the same work (IV 26ff., 318 b ff.) with reference first to διότι and καθότι, which Apollonius does not want to treat as single words, and then to verbs of the καταγράφω type, which for Apollonius are compounds and not phrases. Apollonius exploits the same principles as before with some additions or modifications. He mentions phonological facts (in Greek *h* can only occur in word-initial position) (IV 28), morphological data (the genitive Νεᾶς Πόλεως shows that Νεὰ Πόλις cannot be a compound) (IV 34), structural patterning (IV 41), and finally he offers a more extensive discussion of the accentual principle (IV 47). The individual elements of a compound may have the same accentual pattern in composition as when they are used independently: cf. Περικλυτός vs. κλυτός. In this case observation of the accent contributes little to our understanding of the grammatical status of the sequence. Yet we also find forms like σύνοιδα contrasted with the simplex οἶδα; in this case the accentual difference clearly indicates that σύνοιδα is a compound. The level of sophistication reached at this point is quite high; the obvious comparison is with the standard contrast drawn in modern textbooks between the English word *blackbird* with the main stress on the first element of the compound and the phrase *black bird* with the main stress on the second word.

For our purposes the interesting result is that in Apollonius, as no doubt in all his predecessors, there is still hesitation in the identification and definition of words. Part at least of these hesitations must have been determined by the conflict between an earlier, more primitive view based on the 'one accent, one word' principle which I have argued was the current 'folk-linguistic' view, and the more sophisticated attitudes developed in the course of philosophical and grammatical discussion. In the Greek tradition continuity is not surprising. At a later date – when we cannot say – the anonymous author of the *de prosodiis*, a pamphlet preserved together with the Techne in some manuscripts and at-

tributed to Dionysius, refers to οὐχ οὕτως, i.e. to a phrase formed by a negative particle and an adverb, as to μία λέξις 'one word'. The scholiast, a more sophisticated scholar, reproaches the author for this mistake: οὐχ οὕτως is two words, not one (*Gramm. Gr.* I 1, p. 112–3 Uhlig). Obviously the discussion continued. Another scholiast in his commentary to the Techne produced a fuller definition of word which anticipates much of the discussion of the twentieth century: "a word is the smallest linguistic sound sequence (φωνὴ ἐγγράμματος), which is indivisible, can be said by itself, understood by itself, and is pronounced with one accent and one breathing(?)" (Schol. Marc. in *Gramm. Gr.* I 3, p. 352 Hilgard).[33] We find here the antecedents of more than one modern definition: the word as an accentual unit, the word as an indivisible unit, the word as a minimum free form, the word as a unit of meaning, etc. In this form the scholiast's definition is eclectic and unworkable (though no more so than most of this century's definitions); yet it gives witness of the long and lively discussions which led to it, and perhaps of the 'pre-scientific' 'folk-linguistic' period which preceded these discussions.

9. The general conclusion is brief. During most of its history the ancient Greek lexicon offers no exact equivalent for our 'word', i.e. it has no word with a comparable range of meanings. I have argued that some notion of word was nevertheless available and found its expression in the various forms of writing used in Greece from the Second Millenium onwards. The exact nature of the notion is of course difficult to establish but it is likely that the scribes or stonecutters identified words mainly on accentual criteria: one main accent – one word. The literary texts are ambiguous and often uninformative, not so much because they do not confirm our belief that some concept of word was available (that they do), but because they give us little, if any, information about the nature of this concept. Yet, Plato, Aristotle, and, much later, the grammarians show signs of a deeply ingrained view of the word as accentually identified. Philosophical and grammatical thought challenged or refined this view but I suspect that we ought not to underestimate its importance. In this particular case at least it may be legitimate to argue for some form of continuity between folk-linguistics and linguistics.[34]

Notes

1 For the Homeric words rendered with 'word' cf. Fournier 1946: 211ff.; cf. also Hofmann (1922). In Homer λόγος (in the plural) is attested only twice with the meaning 'words, speech'; the semantic 'expansion' of λόγος probably happened later.

2 In Aristotle, where the word closest in meaning to 'word' is ὄνομα, Bonitz (*Index Arist.* s.v. λόγος I 2) recognizes three passages where λόγος = 'word'. In two of these (*Nich. Eth.* 1103 b; *Soph. El.* 169a) the meaning is ambiguous (e.g. ἐνὶ δὴ λόγῳ may be translated with 'in one word' but in fact refers to a sentence or a phrase); in *Rhet.* 1406 a λόγος seems to refer to one word, a compound, but could well mean something like 'expression'.

3 It is also worthwhile to look at the metalinguistic vocabulary of the Derveni Papyrus which must count as the earliest literary commentary we have in Greece (see for a provisional edition ZPE 1982 and cf. West 1983: 75ff.; Burkert 1985): ἔπος means 'verse', λόγος seems to have a generic value, ὄνομα means 'name', ῥῆμα probably means 'expression' and once (col. xxii 8) refers to one word. Of Demetra it is said that Δημήτη[ρ δὲ] ὠνομάσθη ὥσπε[ρ] ἡ Γῆ Μήτηρ, ἐξ ἀμφοτέρων ἕ[ν] ὄνομα (col. xviii 9–10).

4 For a classical discussion of the notion of word, cf. Bloomfield (1983: sections 1.5 to 11.7). For more modern references and discussions cf. e.g. Allen (1973: 22ff.), Matthews (1974: 20ff. and 159ff.), and the two book-length accounts by Krámský (1969) and Pulgram (1970). An elementary

but very clear discussion is that by Palmer (1971: 41ff.) who reaches no positive results: "In conclusion, sadly, we have to say that the word is not a clearly definable linguistic unit" (1971: 51).

[5] The oldest Greek document from Cyprus is now a syllabic inscription of the eleventh century B.C. with one Greek name in the genitive; cf. Karageorghis (1980: 122ff.). For the Cyprian texts in general cf. Masson (1983).

[6] Exact references are not always given here but can be easily traced through Olivier-Godart (1973), Ventris and Chadwick (1973: 527ff.) and, for the first part of the alphabet, through Aura Jorro (1985).

[7] For the Mycenaean prepositions see Morpurgo Davies (1983), where, however, the accentuation is not discussed. – I have not mentioned above Myc. *dwo* 'two' which seems to contradict the rule according to which graphic words composed of one sign only are not written in isolation. This spelling occurs in Pylos only and the *dwo* sign is probably an invention of a Pylos scribe; since it is formed by two *wo* signs facing each other it may well be that the sign was felt to have a different graphic status from that of e.g. the simple sign *pa* used to write the word for 'all' in the phrase *to-so-pa*.

[8] Cf. Masson (1966), Dressler (1968) and more recently Dunkel (1981: 214ff. with note 2). The interpretation of KN *a-mo-ra-ma* as an iterative *āmorāmar* (cf. Gr. ἦμαρ 'day') is too doubtful to be used in this context.

[9] Dunkel (1981) has argued that for Indo-European we can only reconstruct iterative compounds based on preverbs. If so, the *we-te-i-we-te-i* formation cannot be inherited and one cannot quote the accentuation of Vedic *divédive* etc. as a parallel. Nevertheless composition and accent simplification may have occurred independently in Vedic and Greek. Moreover the Cyprian *a-ma-ti-a-ma-ti* 'every day' discussed by Masson (1966) is also written as one word in an archaic inscription (seventh or sixth century) where word-division indicated by a dot seems to be relatively regular.

[10] I ignore *ka-ka re-ą* of KN R 1815 (written over erasure). A possible explanation is that by Risch (1983: 384) but no word divider is present and it is possible that what the scribe had in mind was **ka-ka-a-re-a* (cf. for the formation Ruijgh 1983: 395f.).

[11] Cf. e.g. Quirk and Greenbaum (1973: 1019). – Though the compounds were normally treated as single words it is also true that the Mycenaean scribes 'responded' to compounds in a different way from that in which they responded to *simplicia*. This was demonstrated (though with different purposes) by Ernst Risch (1983: 374ff.) who succeeded in showing that the scribes adopted special spellings (sometimes prompted by etymological considerations) in order to mark off the different elements of a compound. The importance of Risch's work for a study of Greek attitudes to language is very great.

[12] The odd mistake is attested: cf. *te-ko-to-na-pe* (vs. *te-ko-to-a-pe*), *i-qo-na-to-mo*, *ko-to-na-no-no*; see Risch (1983: 378). Obviously in addition to syllabification questions of juncture may also be relevant.

[13] I have deliberately spoken of monosyllabic proclitics and enclitics because the position of disyllabic appositives, if they existed, is doubtful. The first person singular of the verb 'to be', *e-mi*, is normally separated in writing from the word which precedes, but Masson (1981: 647, note 78) now quotes one example of *e-mi* written in *scriptio continua* with the preceding word and discusses a Kafizin (hence late) instance of ἐτι written in the same manner.

[14] A possible exception is provided by the recently published texts of Kafizin (Mitford 1980: nos. 118 b, 133 b, 191, 303) of the late third century B.C. As shown by Masson (1981: 648), they regularly write *e-se* (ἐς), *ta-se* (τας), and even *ka-se* (κας), but in the passages quoted above they have *ka-se-i-ko-so-to-i* or the like for the expected κας εἰκοστο- instead of the required **ka-se-e-i-ko-so-to-i*. We are prevented from reading κας ἰκοστο-, which would cause no orthographic problems, by the spelling *ka-e-i-ko-so-to-i*: κα εικοστωι of no. 252. It is difficult to understand what is happening, all the more so in view of the 'regular' spelling of the other *-s* prepositives.

[15] Compounds are regularly written as single words with the exception of *e-xe o-ru-xe* found four times in the Idalion inscription (Masson 1983: no. 217, 12 [twice], 24, 25); there may be graphic or phonetic reasons for this spelling (cf. Masson 1983: 241) but we do not understand them.

[16] We should not forget that that of proclitic is a modern and not an ancient concept (cf. e.g. Schwyzer 1939: 350ff.). It is also noticeable that in Cyprian proclitics which end in a vowel call for special syllabification rules (Masson 1983: 69); cf. *to-sa-ta-si-wo-se*: τὸ Σταϝος, which implies that the whole *st-* cluster is assigned to the first syllable of the name and is not divided between the first and the second syllables of the word-group. Linguistically this is not extraordinary. Morphological factors

can influence syllabification; cf. for instance, the Attic scansion of ἐκ-λείπω with a long first syllable, though normally clusters of *muta cum liquida* are assigned to the second syllable (Snell 1958; Ancher 1978: 66f.). More interesting is the fact that the Cyprian spelling may reveal some awareness of the phenomenon.

[17] The origins of punctuation must be Semitic (cf. Larfeld 1914: 302ff.). For our purposes, however, a discussion of the Semitic antecedents is not necessary since the linguistic response in which we are interested is Greek and Greek only. Similarly, it did not seem worthwhile to investigate the use of the 'word-divider' in Cretan scripts other than Linear B.

[18] Still useful, in spite of its date, is the Berlin dissertation by Rudolf Kaiser (1887) with the earlier literature. Some detailed comments are also available in Larfeld 1914: 302ff.; 1907: 564ff. and for a brief discussion see also Guarducci 1969: 391ff. For a preliminary study I found essential the archaic inscriptions collected in Jeffery (1961), where the various forms of interpunction used in archaic Greece are discussed in detail.

[19] See Kaiser (1887: 23ff.), Threatte (1980: 82–3). For some non-Attic examples cf. e.g. the seventh century base from Naxos with μ ἀ:νεθέκε (Schwyzer 1923: no. 757; Jeffery 1961: 304, no. 3, pl. 55) and the fifth century block from Ephesos with δίκαξον:τες (Schwyzer 1923: 462, no. 708, Jeffery 1961: 344, no. 55b).

[20] For the new stele cf. Herrmann (1981); for the old stele (now lost) see e.g. Schwyzer (1923: no. 710); Meiggs and Lewis (1969; no. 30) and for a full list of the old editions cf. Herrmann (1981: 1–2, notes 2–8).

[21] The importance of the Teiae Dirae for a study of interpunction was underlined by Kaiser (1887: 17–19) who drew the right conclusions: "In universum igitur statuere licet, eam esse rationem, ut omnes illae voculae encliticae atque procliticae interpunctione a nominibus suis non seiungantur" (cf. also Larfeld 1914: 303). For the punctuation it is customary to refer to Jacobsohn (1909: 107 note 1), but Jacobsohn has far less to say than Kaiser (1887) or Larfeld (1914). – One word is necessary about the standard editions of the old stele. Interpuncts are sometimes omitted in Meiggs-Lewis (1969: no. 30; cf. B 9, 29, 32, 39 twice) and in Schwyzer (1923: no. 710; cf. A 1, B 31, 32); Schwyzer also adds an interpunct in B 8. Since the stele is lost it is now necessary to return to Boeckh CIG 3044 and Roehl IGA vi. 12. In the new stele Herrmann's edition (1981) shows no interpuncts in a) 21; b) 10, 11; c) 4, possibly d) 12, d) 22. The photographs make it likely that there were interpuncts in c) 4, d) 22 and possibly in b) 10. The other passages are too broken to be checked, with the possible exception of d) 12 where ὅστις δὲ τιμοχέων does not seem to be punctuated after δὲ – nor am I certain that we would expect it to be.

[22] There are two interesting sets of exceptions in Jeffery 1961: 168 no. 8 (= SEG XI 314, XXII 263). First, in a list of demiourgoi which consists of six names joined by τε καὶ interpuncts regularly precede τε and follow και (all instances of τε are at the end of a line, so that there may have been 'potential' interpuncts after τε too). This contrasts with the rest of the inscription, where και occurs twice without being followed by an interpunct and τε occurs once immediately after a noun (with no interpunct) and before interpunct + και. The odds are that the interpuncts are meant to emphasize the names of the demiourgoi. Secondly, the dative plural of the article τοῖσι appears three times before an interpunct (and a noun or another word). It is not sufficient to explain this with the disyllabic form of the article, since the disyllabic preposition ἐπί is not followed by an interpunct. It is conceivable that this form was orthotonic as indicated by the conventional accentuation.

[23] Threatte (1980: 23) observes that "in a number of texts no real rationality seems to lie behind the placing of the interpuncts". This is occasionally so, but there is no reason to list among these texts, as Threatte does, IGAA p. 143, no. 57 = SEG 10.461 (= Hansen 1983: no. 27). The text is punctuated as one would expect for word-division; the only problem is that one colon is missing in the phrase παρὰ σῆμα θανόντος.

[24] So far I have deliberately refrained from mentioning the Near Eastern evidence for word-division, since the question of possible influences is obviously insoluble and in any case is not likely to be important (cf. above note 17). Yet in most cuneiform writings the use of determinatives and logograms implies some concept of word; in cuneiform Hittite there is the added factor that the *Glossenkeil* marks off a word rather than a phrase. Akkadian does not normally separate words in writing but Hittite leaves empty spaces between words and significantly for our purposes treats as one word a sequence of an orthotonic element and any number of enclitic particles. In the First Millennium B.C. (the Second Millennium data are too obscure) the Hieroglyphic Luwian script separates words

with a special sign which in some texts is used with absolute regularity. There too the enclitic particles are not separated from the orthotonic element which precedes them. Finally, we should not forget the extensive grammatical work done in the Near East at a much earlier date than in Greece (and in India). The peculiar linguistic situation created a need for glossaries and these presupposed some notion of word. Also the Old Babylonian grammatical texts of the Second Millennium B.C. give evidence for the setting up of paradigms which presumably presupposed both an analysis into word-forms and some notion of lexeme (cf. Jacobsen 1974: 41ff.).

[25] *Gramm. Gr.* I 1 p. 22 Uhlig: λέξις ἐστὶ μέρος ἐλάχιστον τοῦ κατὰ σύνταξιν λόγου. I have followed Robins' translation (1957: 91); a more literal version is that by Arens (1969: 23): "Das Wort ist der kleinste Teil des auf Zusammenfügung beruhenden Satz".

[26] For an introduction to the question see Pinborg (1975: 69ff. and especially 103ff.). A list of selected recent additions to his bibliography ought to include at least Siebenborn (1976, with the review by Fehling 1979), Erbse (1980), Ax (1982). For the parts of speech cf. also Wouters (1979: especially 179ff.) and Schenkenveld (1983). For the outsider the main problem is that the whole chronology of traditional grammar is now under discussion and that it is difficult to know whether the old view according to which grammatical analysis had reached its traditional form well before the second-first centuries B.C. is still correct.

[27] The odds are that here λέξις does not mean word or only word; cf. Pinborg (1975: 97) and see also the old observations by Schmidt (1839: 18ff. with notes 31, 32, 37); cf. also the German translation by Hülser (Schmidt 1979: 107, note 31 a).

[28] There is a further problem here since Aristotle uses πτῶσις not only for inflection etc. but also for what we would call derivation: in *Rhet.* 1410 a the adjective χαλκοῦς, 'made of bronze' and the genitive χαλκοῦ of χαλκός, 'bronze' are treated as πτώσεις of the same ὄνομα. We would say that χαλκοῦς and χαλκός are different words or lexemes. The literature about Aristotle's πτῶσις is immense; for a survey see Calboli (1972: 86ff.), but cf. also Belardi (1979: 11ff.).

[29] I am grateful for this reference to my colleague Lesley Brown.

[30] For a general discussion cf. Ackrill (1963: 117ff.) and above all Belardi (1979) who rightly disagrees with the views previously expressed by Pagliaro (1954: 29ff.).

[31] This would be difficult to argue if, on the basis of the controversial passage *Soph. El.* 166 b 3 (cf. *poet.* 61a23), we had to assume that Aristotle treated the οὐ negative as oxytone (see e.g. Gudeman's commentary to the relevant passage of the Poetics). Yet I fail to see why translators and commentators ignore Wackernagel's arguments (1955: 1077ff.) in favour of an 'oxytone' οὔ and a 'baritone' οὐ; in the absence of a proper refutation of what seems to me the only correct interpretation I see no need to be worried by these passages and I am ready to argue that for Aristotle οὐ was unaccented (cf. Erbse's note in his edition of the scholia to Hom. *Il.* 23, 238). I do not think that this view must be rejected on the strength of Herodian's statement (I 494, 504 Lenz) that the οὐ negative ὀξύνεται like ἰδού and ἰού (cf. Allen 1973: 253) since the grammarians always spoke of the proclitics as oxytone (Vendryes 1945: 63ff. and 73). The observation that τοῦτο δὲ (scil. οὐ) καὶ ἐν τῇ συνεπείᾳ ὀξύνεται (Herod. I 504) is difficult to understand and cannot be given too much weight particularly since συνεπείᾳ is a modern correction for συνηθείᾳ of the manuscripts.

[32] Obviously the whole of my paraphrasis and summary is coached in terms which are far too modern. I must repeat again (cf. note 15) that 'proclitic' is a modern term (but for the notion of proclisis cf. the observation by Quintilian 1 5 27 quoted by Allen 1973: 25: "cum dico *circum litora*, tanquam unum enuntio dissimulata distinctione, itaque tamquam in una voce una est acuta").

[33] Λέξις ἐστὶν ἐλαχίστη φωνὴ ἐγγράμματος ἀμερὴς ἰδίᾳ ῥητὴ καὶ ἰδίᾳ νοητή, ὑφ' ἕνα τόνον καὶ πνεῦμα ἀγομένη. I have hesitated in translating φωνὴ ἐγγράμματος, the phrase used by Diogenes of Babylon in his definition of λέξις (cf. p. 272 above and note 26; cf. also [Plato] *Def.* 414 d) but it seems likely that by this stage ἐγγράμματος need not mean literally 'which is written' or 'which can be written' but is closer to 'articulate' or 'linguistic'; for an earlier period see already Apoll. Dysc. *synt.* 3, 180 where οὐκ ἐγγραμμάτων <φωνῶν> refers to meaningless sounds or noises.

[34] This article was completed in 1983; except for a very few items it has been impossible to introduce additions and alterations after that date.

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On Vowel Length in Gothic

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Because Gothic is the oldest Germanic language for which we have extensive data, the study of its grammar – including its phonology – has long exerted a special fascination not only for Germanists but also for Indo-Europeanists. I therefore hope that a discussion of vowel length in Gothic is a fitting contribution to this volume written in honor of Henry Hoenigswald – a great Indo-Europeanist who also has a lively interest in Germanic. I shall begin with a review of the scholarship on my topic. This is a small but nevertheless interesting chapter in the history of linguistics: as successive theoretical approaches have changed. so also have interpretations of the Gothic vowels. In conclusion I shall try to make a contribution of my own, using a theoretical approach which is by no means new but which has thus far been largely overlooked.

Until the mid- 1950's it was taken for granted that vowel length was relevant in Gothic, just as in the other older Germanic languages and in reconstructed Proto-Germanic and Proto-Indo-European. A rather widely accepted phonetic interpretation of the graphic evidence was as follows (Streitberg 1920):

Short.	Graphic:	a	u	i			ai	au		
	Phonetic:	[a	u	i			ɛ	ɔ]		
Long.	Graphic:	a	u	ei	e	o	ai	au		
	Phonetic:	[a:	u:	i:	e:	o:	ɛ:	ɔ:]		
Diphthongal:	Graphic:						ai	au	iu	
	Phonetic:						[a$_{\underline{i}}$	a$_{\underline{u}}$	i$_{\underline{u}}$]	

> For ease of typing and printing, I shall symbolize vowel length by writing a colon after a vowel symbol rather than a macron over it – regardless of the practice of the authors whom I cite. This has the added advantage of reminding us visually that, in terms of syllable structure, long vowels function as the equivalent of short vowel plus consonant, i.e. /V:/ = /VC/. This is very astutely pointed out in Vennemann 1971: 106–7.

In order to symbolize the three assumed phonetic values of graphic *ai, au*, philologists (following Jacob Grimm) customarily wrote them as follows: (1) short monophthongal *aí, aú*, reflecting earlier short vowels, as in *taíhun* 'ten', *saíhʷan* 'to see', *baíran* 'to bear', *daúhtar* 'daughter', *haúrn* 'horn'; (2) diphthongal *ái, áu*, reflecting earlier diphthongs, as in *stáins* 'stone', *áugo* 'eye'; (3) long monophthongal *ai, au*, reflecting earlier long vowels (before a following vowel), as in *saian* 'to sow', *bauan* 'to build'. The strongly historical-comparative nature of this three-way interpretation of graphic *ai, au* is obvious.

Where there was disagreement with this interpretation, it concerned not the assumption of vowel length but rather the assumption of three different phonetic values each for graphic *ai, au*. An example is Wright 1910. Throughout the text of his grammar he used the traditional interpretation and the following spellings: short *a, u, i, aí, aú*; long *a:, u:, ei, e:, o:, ai, au*; diphthongal *ái, áu, iu*. But he was skeptical. At the end of his grammar he wrote: "It seems almost incredible that a man like Ulfilas, who showed such skill in other respects, should have used *ai* for a short open *e*, a long open *æ*: and a diphthong; and *au* for a short open *o*, a long open *ɔ*: and a diphthong. Whereas, if we assume that the diphthongs had become monophthongs (*æ:*, *ɔ:*), there is nothing incongruous in his having used each of the digraphs to represent two sounds which differed in quantity but not in quality" (362). That is to say, he believed it more likely that Gothic had the following vowels (with minor changes in symbolization):

Short.	Graphic:	a u i ai au
	Phonetic:	[a u i ɛ ɔ]
Long.	Graphic:	a u ei e o ai au
	Phonetic:	[a: u: i: e: o: ɛ: ɔ:]
Diphthongal:	Graphic:	iu
	Phonetic:	[iu̯]

Neither Wright nor any other philologist, however, doubted that Gothic showed a distinction between short and long vowels.

In the first "structuralist" treatment of Gothic phonology (Moulton 1948), I used for vowels essentially the former of these two interpretations. Though my article was badly flawed in its treatment of the vowels (in retrospect I have often wished that I had never published it), it had at least one advantage: it stimulated a number of other structuralists to write articles showing how wrong I had been. Reactions were not long in coming. In 1949 Bennett published an article on "The monophthongization of Gothic *ái áu*." After referring to scholars who accepted the traditional view that graphic *ái, áu* still represented diphthongs at the time of Wulfila, he wrote: "In recent years, however, an increasing number of linguists, including Hirt, Marstrander, Wright, and Mossé [references] have reached the conclusion that Wulfila's *ái áu* were no longer diphthongs but had become monophthongized to long open [ɛ: ɔ:] respectively" (15). He then gave arguments for this view, which convinced me then and still convince me now. He did not, however, question the relevance of vowel length.

The second reaction came in a 1950 article by Penzl, "Orthography and phonemes in Wulfila's Gothic." He, too, assumed that traditional *ái, áu* had been monophthongized to long /ɛ:/, /ɔ:/, but he also did something more important than this. In my 1948 article I had assumed that some of Wulfila's spellings reflected allophones. Penzl not only showed that I was mistaken; he also formulated a general principle. Speakers are not generally aware of the allophones in their native language; hence only under very special conditions can we expect them to symbolize allophones in writing. Applying this reasoning to Gothic, Penzl wrote: "If Wulfila's orthography was created by a native speaker of Gothic and was intended for other native speakers of Gothic, any representation of subphonemic variants must seem somewhat surprising and demand a special explanation. . ." (217). This reasoning convinced me; I should have known better. The final sentence in Penzl's article reads as follows: "It cannot be emphasized enough that Wulfila's orthography and its relation to

the phonemic pattern must be the main consideration in any study of the phonemes of his Gothic dialect" (230).

The implications of Penzl's article are clear. Except in most unusual circumstances, the orthography of a language will not indicate any subphonemic (allophonic) distinctions; it will indicate **only** phonemic distinctions. To this there is a corollary. Can we also assume that the orthography will indicate **all** phonemic distinctions; and, if a possible distinction is **not** indicated in the orthography, is this proof that it did not exist? Four years later Penzl (1954: 412) asked just this question: "Can we actually prove that [vowel] quantity is phonemic in Wulfila's Gothic?" Though he raised this question, he did not attempt to answer it.

An answer was not long in coming, namely in a 1955 article by one of Penzl's students, Marchand. After reviewing the evidence, Marchand concluded his article by writing: "It is believed that a case could be made to the effect that [vowel] length was not phonemic in Gothic, though the point cannot be pressed too far. . . If anyone states that length was or was not phonemic in Gothic, he should realize that this is, to a great extent, an arbitrary statement" (86). (Cf. also Marchand's 1955 dissertation, later published in 1973.)

Marchand's cautiously noncommittal statement concerning the relevance of vowel length in Gothic was soon accepted more or less unquestioningly by a number of other structuralists, notably Jones 1958 and Hamp 1958 (these were successive articles in volume 34 of *Language*) and Bennett 1964. As good structuralists, they then proceeded to describe the full vowel "system" of Gothic, rejecting length as phonemic. This was done in the greatest detail by Marchand (1958: 508):

		Front	Central	Back
High	Close	i (ei)		u
	Open	ɪ		
Mid	Close	e		o
	Open	ɛ (ai)		ɔ (au)
Low			a	

The one remaining problem was how to integrate graphic *iu* into this system; various solutions were proposed.

With the advent of a new approach to phonology, that of generative phonology, there came a new series of works on Gothic; and, as we might expect, the generativists have been just as fascinated by the phonology of Gothic as were the structuralists. Two of these works have appeared only as dissertations, namely Buckalew 1964 and Beade 1971; and – inexcusably – I have not read them. (I know only, from Vennemann 1971 and Beck 1973, that Buckalew rejected vowel length in Gothic, but I do not know his reasons for doing so.) The one generative work that I have read in which vowel length is rejected is Wurzel 1975. His reasons for doing so are essentially the same as those of the structuralists: "Wir können also unterstellen, daß die Orthographie des Ulfilas die allgemeinen Prinzipien, die jeder überlegten Neuschöpfung von orthographischen Systemen zugrunde liegen, zur Richtschnur hat. . . (I) Die Schreibung gibt phonetische Distinktionen dann und nur dann wieder, wenn sie kontrastieren. . . (II) Dieses Prinzip ist Grundlage für die Schaffung aller Buchstabenorthographien und bei neugeschaffenen orthographischen Systemen auch entsprechend befolgt. . .

(III) Die verwendeten Schriftzeichen werden dabei – falls sie einem bereits existierenden Schriftsystem entstammen – soweit wie möglich mit dem phonetischen Wert benutzt, den sie als 'normalen' Wert in ihrem Herkunftssystem besitzen. . ." (266–7). Wurzel's bibliography suggests that he was not familiar with Penzl 1950; his "general principles," however, are essentially the same as Penzl's.

The earliest generative work that I have read is Voyles 1968, and he assumes that vowel length **was** relevant in Gothic. His argument is as follows: "In any event, Wulfila seems generally to have disregarded length in the formulation of his orthography – except in the single case of the representation of /i:/ by *ei*. This is perhaps ascribable to the prior orthographical traditions of fourth-century Greek and Latin on which Wulfila was in part dependent, and which had no specific signs in general use to mark length or stress. Thus length is not the only distinctive feature Wulfila chose to exclude from his orthography: he also disregarded stress, which was doubtless phonemic in Gothic. . . At least one conclusion may be drawn from this: Wulfila seems to have ignored the suprasegmentals in his otherwise more-or-less phonemic analysis of Gothic phonology" (722). Except that I find it less probable that an orthography will indicate stress than that it will indicate vowel length, I find this argument very persuasive. At the same time, however, it is in a sense merely a return to previous reasons for believing that vowel length was relevant in Gothic. In particular, it is not based on any new insights derived from the methodology of generative phonology. (Vowel length is also accepted in Voyles 1981.)

Very different from this is Vennemann 1971. Here the methodology of generative phonology **is** used to provide evidence that there were two classes of vowels in Gothic, and that they differed almost certainly as short vs. long. Though it is impossible to summarize in a few paragraphs his long article (43 pages) and his very careful reasoning, I can at least describe some of those parts of it which deal with vowel length.

It is generally accepted that Gothic shows a "lowering rule," whereby certain vowels – but not others – appear lowered before graphic *h*, *h*^{w}, *r*. In order to give a generative accounting for this, Vennemann suggests, let us assume a distinctive feature F (whatever it may turn out to be) with plus and minus values; and let us assume that vowels which do **not** show this lowering are +F, whereas vowels which **do** show this lowering are -F. Examples of these two classes of vowels are as follows (insofar as possible, morphologically parallel forms are given as illustrations; Roman numerals refer to the traditional classes of strong verbs):

Vowels with +F

Graphic	Most environments	Graphic	Before *h*, *h*^{w}, *r*
ei	greipan (I, infin.) galeiþan (I, infin.) skeinan (I, infin.)	*ei*	gateihan (I, infin.) leih^{w}an (I, infin.) skeirs
u	brukjan (infin.) fuls	*u*	bruhta (pret.) skura
e	qeþun (V, pret. pl.) gebun (V, pret. pl.) nemun (IV, pret. pl.)	*e*	frehun (V, pret. pl.) seh^{w}un (V, pret. pl.) berun (IV, pret. pl.)
o	sokun (VI, pret. pl.) holoþ	*o*	slohun (VI, pret. pl.) horos

Vowels with -F

Graphic	Most environments	Graphic	Before *h*, h^w, *r*
i	undgripun (I, pret. pl.) giban (V, inf.) stiland (IV, pres. pl.) gaswiltan (III, inf.)	*ai*	gataihun (I, pret. pl.) saih^{w}an (V, inf.) bairand (IV, pres. pl.) wairþan (III, inf.)
u	anabudun (II, pret. pl.) andnumans (IV, ppl.) gaswultun (III, pret. pl.)	*au*	tauhun (II, pret. pl.) gabaurans (IV, ppl.) waurþun (III, pret. pl.)

(Vennemann gives far more examples than the above small selection, and the above examples are not necessarily the same as his – though they all illustrate the point he wishes to make. To make sure that all of my examples are actually attested, I have checked them with de Tollenaere & Jones 1976.)

The above examples show that graphic *ei, e, o* symbolized systematic phonemes with +F (not lowered before *h*, h^w, *r*), and that graphic *i* symbolized a systematic phoneme with -F (lowered before *h*, h^w, *r*). More interestingly, they show that graphic *u* symbolized two different systematic phonemes: one with +F as in *bruhta, skura* (not lowered before *h, r*); and one with -F as in *tauhun, gabaurans, waurþun* (lowered before *h, r*). Let us now arbitrarily symbolize +F by writing a colon following the vowel symbol, and -F by not writing a colon. This gives for Gothic the following two (partial) sets of systematic phonemes: /i: u: e: o:/ and /i u/.

A first question that we must now ask is: What was the phonetic nature of the distinctive phonological features +F and -F? We cannot know for sure, of course. Diachronically, however, /i: u: e: o:/ reflect earlier long vowels, and /i u/ reflect earlier short vowels. The most likely answer is therefore: +F = +Long, and -F = -Long. (Vennemann also gives synchronic phonological reasons for this assumption.)

A second question that we must ask is: Was this lowering rule a diachronic rule (no longer operative in Wulfila's Gothic), or was it a synchronic rule (still operative in Wulfila's Gothic)? Vennemann gives reasons for believing that it was "a very low-level process" (99) and hence presumbably "synchronic" (130). Consequently, he does not list [ɛ] and [ɔ] among the systematic phonemic vowels in native words of Gothic (126), because they can be accounted for by the lowering rule; but he does list them among the systematic phonetic vowels (130).

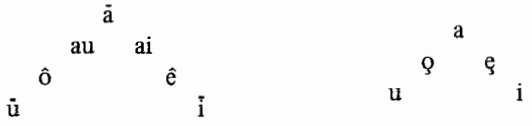
Vennemann summarized, as follows, what I take to be the basic theory underlying his study: "Hypotheses concerning the segment inventory [of a language] have to be based not on a study of the language as reflected in the spelling system, but on the grammar generating that language, especially its phonological component" (127). At the same time, he was fully aware of the historical-comparative reason for assuming that vowel length was relevant in Gothic: "It is not at all plausible that the Germanic language which was most amply documented the earliest should at the same time be the most deviant one and should have leveled a distinction which other Germanic languages have preserved to the present day" (130).

The lively scholarly interest in the phonology of Gothic during the period 1948 to 1981 is shown by the fact that I have been able to locate some forty studies devoted to this topic

during these years – and there may well have been more. Interpretations of vowel length have changed as theoretical approaches have changed: first primary emphasis on the historical-comparative evidence, then primary emphasis on the orthographical evidence, then primary emphasis on the methodology of generative phonology. After such an apparently exhaustive treatment, is it possible to add anything new? I believe that it is, namely by using the approach of yet another theory.

The theory is that of "phonological space." Though scholars have used this theory implicitly for over a century and a half, remarkably little has been written about it explicitly (but cf. Moulton 1962). Vowels, for example, occur in a phonological space that is formed by such familiar "dimensions" as high vs. low, front vs. back, unrounded vs. rounded, short vs. long, etc. The vowels in these interlocking dimensions form for any given language its vowel "system"; and historical changes consist of the "movement" of vowels within the system. As early as 1822 we find Jacob Grimm writing of "sounds" which "occupy a position," "move from a position," "move down one step," and "fill a gap." (For similar ideas found also in the writings of Hermann Paul and Karl Luick, cf. Moulton 1962: 24–5.) The theory of phonological space is also implicit in such common expressions as the "lowering," "raising," "fronting," "backing" of vowels. These terms would be meaningless without the assumption of a phonological space in which such movements can take place.

Though these ideas were presumably well known, philologists did not in general use them to group vowels into a "system." At the beginning of this study I have given the vowels of Gothic just as they are presented in Wright 1910 and Streitberg 1920: not as a "system" but simply as a list – in an apparently arbitrary order. The notion of "system" was, however, obviously of fundamental importance in Trubetzkoy's famous 1929 article, "Zur allgemeinen Theorie der phonologischen Vokalsysteme"; and among the many languages used as illustrations he included Gothic (126). In this first presentation of the vowels of Gothic as a system, Trubetzkoy distinguished two classes of vowels: "maximalintensiv" (given below to the left) and "minimalintensiv" (given below to the right). He displayed the whole system graphically as follows:



(Trubetzkoy interpreted /au ai/ as falling diphthongs. Note that graphic *iu* does not appear in this system.)

Then came the American structuralists. Since the notion that phonemes form "systems" (or "structures") was fundamental to their theory, they soon began to present the vowels of Gothic as forming a system; an example is the system given in Marchand 1958: 508 (noted above), where the distinctive features that give the dimensions of the system are fully specified. Then came the generative phonologists, who presented the vowels of Gothic as a system of a very different sort: not as a set of elements occupying positions within phonological space, but as a set of elements composed of distinctive phonological features. Cf. Voyles 1968: 724, where the vowels (underneath them the distinctive features of which they are composed) are listed linearly in alphabetical order – first short, then long. The reason for this linear order is clear. The methodology of generative phonology is not

designed to describe sounds as elements of a system within phonological space, and individual sounds as elements which (to quote Jacob Grimm) "occupy a position" within phonological space. In addition, the methodology of generative phonology was not designed to describe sound change as the "movement" of elements within phonological space, where the elements may (to quote Grimm again) "move from a position," "move down one step," or "fill a gap." (Without the theory of phonological space there can of course be no such things as movements, positions, steps, or gaps.) Instead, sound change consists of changes in distinctive features. This means that the methodology does not describe change as such ("change-in-progress") but only the **results** of change, as in the formula A → B/X (feature A changes to feature B in the environment X). What this formula tells us is: before the change, a given segment contained feature A; after the change, it contained feature B; but the change as such is not described.

> Vennemann (1971) seems to be an exception among generative phonologists in that he accepts the notion of "system." On p. 127 he gives the "vowel inventory of Gothic" in the usual generative fashion: as a list of segments in a more or less arbitrary linear order. However, on pp. 126 and 130 he displays first the two sets (short vs. long) of systematic phonemic vowels and then the two sets of systematic phonetic vowels as "systems" in a two-dimensional space composed (though not explicitly so identified) of the dimensions high – low and front – back, in the usual configuration: high at the top, low at the bottom; front to the left, back to the right. There is only one slight violation of phonological space: [ɛ ɔ] and [ɛ: ɔ:] are written as if they were as low as [a] and [a:]. I welcome this implicit use, by a generativist, of the theory of phonological space: it will simplify the remarks that I shall make shortly.

There are three phenomena in Gothic to which the theory of phonological space can be applied so as to show that Gothic clearly had two sets of vowels – most likely distinguished as short vs. long. All three phenomena have long been recognized. They are as follows:

(1) The lowering of earlier **i* and **u* before graphic *h*, h^w, *r*, as described above. (Note again that the very term "lowering" implies the theory of phonological space.)

(2) Graphic alternation between *u* and *au*, especially in *u*-stem nouns. This has been most extensively investigated by Marchand (1956). He notes: 27 examples of *au* for expected *u*, and 32 examples of *u* for expected *au*. (This is a gross simplification of his far more sophisticated presentation.)

(3) Graphic confusion of *ei, i, e.* (Again I am simplifying Marchand's very sophisticated presentation.) We find:

(a) 65 examples of *ei* for expected *e.*
(b) 38 examples of *e* for expected *ei.*
(c) 20 examples of *i* for expected *e.*
(d) 16 examples of *e* for expected *i.*
(e) 5 examples of *i* for expected *ei.*
(f) 4 examples of *ei* for expected *i.*
(g) 10 examples of *o* for expected *u.*
(h) 4 examples of *u* for expected *o.*

Let us now consider how these three phenomena might be described in terms of the theory of phonological space – where sounds are elements within a "system," "occupy a position" within the system, but may also "move" from one position to another within the system. In each case I shall consider two alternative proposals: (A) that vowel length was

not relevant in Gothic; (B) that vowel length **was** relevant in Gothic. For (A) I shall use the system explicitly described in Marchand 1958. For (B) I shall use the systems implicitly presented in Vennemann 1971 (but I shall indicate /a/ and /a:/ as lower than /ɛ ɔ/ and /ɛ: ɔ:/).

Let me repeat first the different correspondences between graphemes and phonemes assumed in these two proposals:

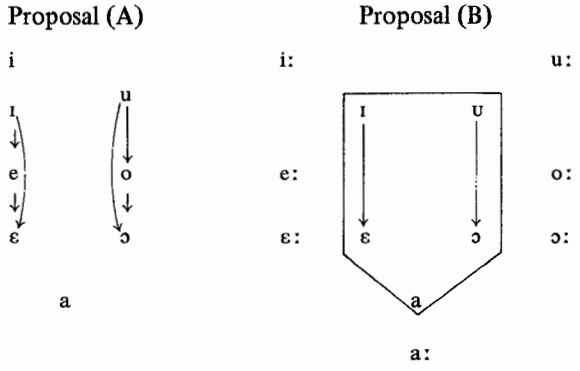
(A) Length not distinctive: *ei i e ai a au o u*
/i ɪ e ɛ a ɔ o u/

(B) Short: *i ai a au u* Long: *ei e ai a au o u*
/ɪ ɛ a ɔ u/ /i: e: ɛ: a: ɔ: o: u:/

In proposal (B) I shall suggest that the long vowels were also tense, and hence farther from the center of the spatial diagram; and that the short vowels were also lax, and hence closer to the center of the spatial diagram. This is by no means a necessary assumption. The diagram must merely show that, in addition to the dimensions high – low and front – back, there was also the dimension short – long. I shall show this by placing the system of short vowels inside the system of long vowels, and by drawing lines around it. I suggest that the long vowels were tense and the short vowels lax merely because this is what we typically find in systems with both long and short vowels. Vennemann (1971: 129) also considers it "very likely" that the long vowels were (redundantly) tense, the short vowels (redundantly) lax.

We can now consider how the three phenomena just mentioned might be described in the phonological space of the single system of proposal (A) and in the phonological space of the two systems (long and short) of proposal (B).

(1) Lowering of **i, *u* before graphic *h, h^w, r.*

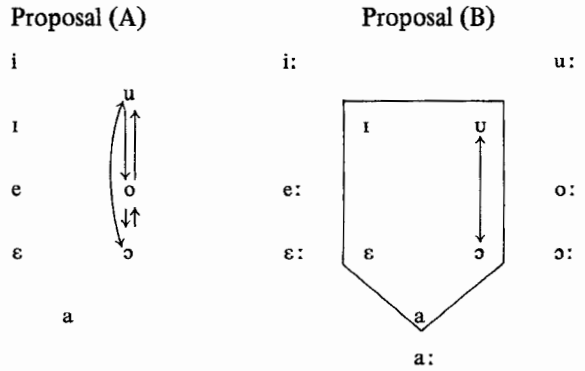


In terms of the theory of phonological space, the lowering of **i, *u* under proposal (A) is highly implausible. First, the lowering of **i*. On the one hand, we might assume (straight arrows) that /ɪ/ was lowered to and merged with /e/; we would then have to assume that all words with /e/ somehow became disentangled so that only those from earlier /ɪ/ were lowered further to /ɛ/, whereas those from earlier /e/ remained unlowered. Such a "disentanglement" is quite implausible. Alternatively, we might assume (curved arrow) that, in being lowered to /ɛ/, earlier /ɪ/ somehow bypassed intervening /e/. This, however, contradicts the whole notion of "movement" within phonological space. The lowering of /u/

under proposal (A) is even more implausible. First, we would have to assume that the /u/ of some words was lowered, whereas the /u/ of other words was not. As for the words in which /u/ was lowered, we have the same difficulties as above: either (straight arrows) a lowering to and merging with /o/, and then somehow a disentanglement so that only the /o/ of words with earlier /u/ was lowered further to /ɔ/, whereas the /o/ of all other words remained unlowered. Or, alternatively (curved arrow), the /u/ of those words which show lowering somehow bypassed the intervening /o/. This again contradicts the whole notion of "movement" within phonological space.

In contrast, the lowering of **i, *u* under proposal (B) presents no difficulties whatever. Within the phonological space of the short vowels, /ɪ ʊ/ simply "moved down one step," giving /ɛ ɔ/. No more needs to be said.

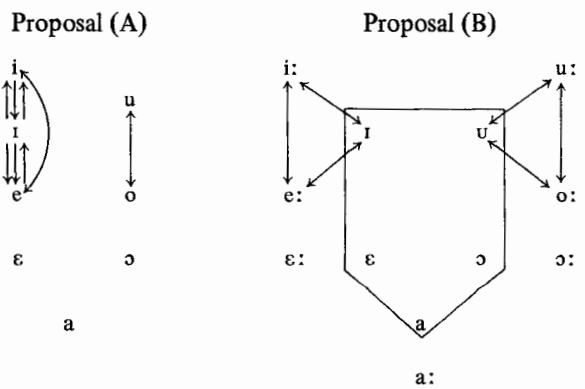
(2) Graphic alternation between *u* and *au*.



The graphic alternation between *u* and *au* in terms of proposal (A) is, if anything, even more implausible than the changes in (1). To explain *au* /ɔ/ for expected *u* /u/ (downward arrows), we would have to assume lowering to and merger with *o* /o/, then disentanglement, then further lowering to *au* /ɔ/. To explain *u* /u/ for expected *au* /ɔ/ (upward arrows), we would have to assume raising to and merger with *o* /o/, then disentanglement, then further raising to *u* /u/. Alternatively, we would have to assume (curved double-headed arrow) bypassing in both directions.

In contrast, the graphic alternation between *u* and *au* under proposal (B) is quite straightforward: it involves merely an alternation in height between two vowels that are neighbors within short vowel space.

(3) Graphic confusion of *ei, i, e*, and *u, o*.



In terms of proposal (A), the graphic confusion of *u* and *o* is entirely compatible with the theory of phonological space: it reflects merely a confusion between two vowels that were neighbors within phonological space. (This is true, of course, only under the assumption that graphic *u* reflects a single phoneme.) The graphic confusion of *i* with *ei* and *e* is also entirely compatible with the theory of phonological space: /ɪ/ moved sometimes one step up to /i/, and sometimes one step down to /e/ (the upward and downward arrows). The graphic confusion between *ei* /i/ and *e* /e/ is far more problematical. One might imagine two "double confusions": /i/ first moved one step down to and merged with /ɪ/, and then, after disentanglement, it moved a second step down to /e/ (the two downward arrows); and /e/ first moved one step up to and merged with /ɪ/, and then, after disentanglement, it moved a second step up to /i/ (the two upward arrows). Yet this seems hardly plausible. There is, of course, the alternative of "bypassing" (curved double-headed arrow), though this is incompatible with the whole notion of phonological space.

In contrast, the graphic confusion of *ei, i, e* and *u, o* under proposal (B) is quite unproblematical (except that we do not understand why this confusion should have come about in the first place). We find in every case a confusion between vowels that were neighbors within phonological space. The confusion may involve two long vowels: /i:/ and /e:/, /u:/ and /o:/. Or it may involve a long vowel and a short vowel: /i:/ and /ɪ/, /e:/ and /ɪ/; /u:/ and /ʊ/, /o:/ and /ʊ/. To show that there is nothing unusual about a confusion (or fluctuation) between a long vowel and a short vowel, we need to search no farther than English: *roof, hoof*, etc. either with long /u:/ or with short /ʊ/.

In view of the evidence from generative phonology (Vennemann 1971) and from the above discussion of phonological space, it seems clear that (disregarding graphic *iu*) Gothic must have had two classes of vowels, and that they were most likely distinguished as short vs. long. We now face a question which plagues us all when we try to describe Gothic. Just what do we mean by "Gothic"? Do we mean (1) pre-Wulfilian Gothic (at whatever period)? Or (2) Wulfilian Gothic (mid 4th century)? Or (3) the Gothic of the scribes who wrote the manuscripts (probably early 5th to late 6th century; cf. Marchand 1958: 497)?

Let us summarize what we know about vowel length at each of these three stages. The rule lowering **i, *u* before graphic *h, h^w, r* was operative at stage (1), pre-Wulfilian Gothic (and Vennemann suggests that it may have been operative down into Wulfilian Gothic). **It requires the assumption of vowel length.** The graphic alternation between *u* and *au* may have occurred during Wulfilian times, but it is more likely that it reflects stage (3), the language of the scribes (cf. Marchand 1956: 148). **It requires the assumption of vowel length.** The graphic confusion of *ei, i, e* and *u, o* almost certainly belongs to stage (3), the language of the scribes (cf. Marchand 1956: 146–7). **It requires the assumption of vowel length.**

This is probably the nearest we shall ever come to solving the riddle of vowel length in Wulfilian Gothic. Two hypotheses are possible:

(a) Vowel length was relevant (1) in pre-Wulfilian Gothic, (2) in Wulfilian Gothic, and (3) in post-Wulfilian Gothic. That is to say, proposal (B) was valid throughout the entire period.

(b) Vowel length was relevant (1) in pre-Wulfilian Gothic. (2) As the orthography shows, it was **not** relevant in Wulfilian Gothic (this is proposal A). (3) Vowel length was relevant in the Gothic of the scribes, though this was clearly a somewhat different kind of Gothic.

So what have we "proved"? Essentially nothing. But let the reader decide.

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Ein Halbvers und einige Epitheta aus vorhomerischer Dichtung*

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Summary

This paper offers new observations on the Ethiopid as a source of the Iliad and on epithets borrowed by Homer from his sources. The known facts: In the Ethiopid Achilles' friend Antilochus saved his father from Memnon's attack and was killed by Memnon and then revenged by Achilles. Surpassing the author of the Ethiopid, Homer invented a new friend for Achilles, Patroclus, and a new adversary, Hector. As Achilles, insulted by Agamemnon, does not fight and thus leaves his friend unprotected, Patroclus is killed by Hector and revenged by Achilles.

Although the parallelism was noticed long ago, the priority of the Ethiopid remained controversial until the origin of the personal names *Patro-kl-os* and *Men-oit-ios* was clarified, thus proving it. In these names Homer evoked his pattern, namely the heroic deed of Antilochus answering his father's cry for help and standing firm, when facing certain death.

With a view to imitating in his epic the great scenes of the death of Achilles and of the following events, Homer transferred these motives to Patroclus as well. For his death he curiously enough combined both his models, i.e. the death of Antilochus and that of Achilles by Apollo and Paris. Therefore he created, debasing Hector's heroic role, two adversaries of Patroclus preceding Hector, Apollo and *Eu-phorb-os*, the latter modelled on Paris, hence his shepherd's-name.

Furthermore an epithet of Patroclus, ἱπποκέλευθος 'finding a way for the horses', also quotes the exploit of Antilochus, who deblocked his father's chariot. Attributed to Patroclus the word has no sense and was accordingly misunderstood by all the interpreters both antique and modern, except by a single scholiast. Here is the crucial point: That scholiast cannot have been able to understand the epithet from the Iliad either. His knowledge must have derived from comments on the Ethiopid, and thus the very word must have occurred in that poem.

Now we can conclude of its context there. On metrical grounds the epithet stood in the vocative case, in a hemistich *Ἀντίλοχ' ἱπποκέλευθε. Such an address can only have been the beginning of a challenge from Memnon to Antilochus immediately before his last fight.

That Homer transferred an epithet from a model to its copy, without regard to its meaning, furnishes a heuristic principle: Others of Homer's epithets may now be in the same class. Indeed χαλκοκορυστής, κορυθαίολος, and ἀνδροφόνος can be proved to be transferred from Memnon to Hektor, δολόεσσα and δεινή from Circe to Calypso, ἱππεύς from Antilochus to Patroclus again. And Homer was not the only one to act like this. The practice was usual among epic poets: In the pre-Odyssean Argonautica for instance the Queen of the Phaeacians, Arete, was called λευκώλενος after Hera, her model.

* Diese Arbeit ist mit Zusätzen, die hier nicht mehr haben aufgenommen werden können, erstmals im MUSEUM HELVETICUM 43, 1986:4 erschienen.

1. Unter den verschollenen Epen vom troianischen Sagenkreis nimmt die *Aithiopis* eine Sonderstellung ein, weil sie, älter als Homers Epos vom Zorn des Achilleus, ihn, Homer, zur überbietenden Nachbildung angeregt hat. Ein Kernstück der Aithiopis erzählte den Opfertod des Antilochos, der seinen Vater Nestor vor dem heranstürmenden Aithiopenfürsten Memnon gerettet hat, aber dabei selber vom weit überlegenen Memnon erschlagen wurde. Ich wage nun zu vermuten, daß Memnon den Nestoriden vor dessen letztem Kampf angesprochen und seine Rede mit den Worten *'Αντίλοχ' ἱπποκέλευθε eingeleitet hat. Hier versuche ich, das glaubhaft zu machen, und werde anschließend die Herkunft einiger Epitheta von Personen Homers aus den Vorlagen des Dichters nachweisen.

2. Zuvor muß an einiges bereits Gesicherte über Homers Inspirationsquelle für seine Menisdichtung erinnert werden. Die Erkenntnis, daß er durch die Aithiopis zu einem eigenen Epos von Achilleus herausgefordert worden ist, hat sich gegen die seit Welcker[1] herrschende Meinung, alle kyklischen Epen seien nachhomerisch, erst in unserem Jahrhundert durchgesetzt. Nach wenigen Vorläufern brachte vor allem das Buch von H. Pestalozzi[2] den Durchbruch. Aber der schlüssige Beweis der Abhängigkeit Homers von der Aithiopis gelang erst auf dem Umweg über die *Namen* der von Homer erfundenen Helden Patroklos und Menoitios, die zusammen mit dem Wort ἱπποκέλευθος auf die hohe Stunde des Antilochos in der Aithiopis Bezug nehmen[3].

3. Man weiß: Achilleus hat zwei Freunde, Patroklos und Antilochos, diesen schon in der Aithiopis, jenen erst bei Homer, der ihn für sein neues Achilleusbild hinzuerfunden hat. Beide Freunde fallen, beide werden von Achill gerächt. Aber in der Aithiopis fiel der Freund in Selbstaufopferung für seinen Vater, ohne alle Mitschuld Achills. Dessen Heldentum war dort, daß er zur Rache an Memnon auszog, obwohl er wußte, daß er selber gleich nach Memnons Tod würde sterben müssen; das hatte ihm seine Mutter Thetis auf Grund von Vorzeichen geweissagt und damit erwirkt, daß er sich des Kampfes gegen Memnon bis dahin enthalten hatte. Von solcher Art also war der Achilleus der Aithiopis, die von den Alten einem Arktinos von Milet zugeschrieben wurde.

4. Homer dagegen, mit seinem Vorgänger ringend, ihn übertreffend, konzipiert einen andern Achilleus, der nicht wegen Vorzeichen und Warnungen vom Schlachtfeld fernbleibt, sondern aus Groll gegen Agamemnon wegen beleidigter Ehre, und der deshalb selber, in seinem unversöhnlichen Starrsinn, durch seine Kampfenthaltung am Tod seines Freundes mitschuldig wird. Für einen solchen Achilleus ersann Homer den Streit mit Agamemnon, die erfolglose Bittgesandtschaft und eben, die Rollen der Vorlage frei nachbildend, den neuen Freund und den neuen Gegner, also an Stelle von Antilochos und Memnon jetzt Patroklos und Hektor. Dabei dichtete Homer so sehr in Auseinandersetzung mit dem Epos seines Vorgängers – oder war jener Dichter gar noch sein Zeitgenosse, sein Rivale im musischen Agon? –, daß er Achills neuem Freund Namen, Vaternamen und Beiwort im Gedanken an den Opfertod des früheren Freundes gab.

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1 *Der epische Cyklus* II (1849) 269ff.
2 *Die Achilleis als Quelle der Ilias* (1945).
3 *Studi Micenei ed Egeo-Anatolici* 9 (1969) 86–93 (mit Literatur S. 91 Anm. 24).

5. Wir kennen nämlich, vor allem dank Pindars rühmender Erwähnung[4], den Hergang jener Heldentat des Antilochos in der Aithiopis ziemlich gut: Paris hatte ein Pferd von Nestors Gespann angeschossen, Nestor war blockiert und hatte, vom heranfahrenden Memnon bedroht, um Hilfe gerufen. Antilochos *hörte* auf den Ruf seines *Vaters* – etwa πατρὸς ἔκλυε –, eilte, aus Pietät allzu kühn, während die andern Achäer flohen, herbei und bereitete dem *Wagen* einen *Ausweg* – ἵπποισι κέλευθον –, indem er die Riemen des verletzten Pferdes durchhieb und es dadurch vom Wagen abtrennte, so daß Nestor wegfahren konnte. Dabei wagte es der Sohn, um den Rückzug des Vaters zu decken, vor dem gewaltigen Memnon nicht zu fliehen, sondern sich ihm in den Weg zu stellen und *stehen zu bleiben* vor dem sicheren *Untergang* – μένειν οἶτον –, und fiel. Deshalb also gab Homer Achills neuem Freund den Namen Πάτρο-κλος[5], das Epitheton ἱππο-κέλευθος und einen Vater Μεν-οίτ-ιος.

6. Es ist denn auch ganz unmöglich, diese Namen und dieses Epitheton etwa auf Patroklos selber sinnvoll zu beziehen. Denn es gibt in Homers Patroklie keine Stelle, wo der Held auf seinen Vater hörte[6], keine, wo er auf den Tod gefaßt einem mächtigeren Gegner widerstünde, keine, wo er gar für irgendwelchen behinderten Kriegswagen einen Ausweg fände. Die Gegenprobe bestätigt es also: Die drei Wörter weisen auf die Vorlage zurück, auf Antilochos und seine große Tat. Nach diesem Befund ist die Umkehrung, die Annahme einer Priorität des Patroklos vor Antilochos, also der Menis Homers vor der Aithiopis im Sinne Welckers und führender Gelehrter nach ihm, nicht mehr denkbar. Aber erstaunlich bleibt dieses Verfahren Homers, hier wie anderswo, seinen Personen Namen und Beiwörter nach ihren Vorbildern zu geben.

7. So hat denn Homer, was er von Achill, Patroklos und Hektor erzählt, zwar zeitlich *nach* dem erfunden, was die Aithiopis von Achill, Antilochos und Memnon erzählte, aber seinen neuen Achilleusstoff im Ablauf des ganzen Achilleusgeschehens notwendig den Inhalten der Aithiopis *vorangestellt*; denn Achills Ende unmittelbar nach seiner Rache an Memnon war dank dem Erfolg der Aithiopis in die Sage eingegangen und zur gültigen "Wahrheit" geworden, wie eben 'Sage' entsteht: aus großer Dichtung, die im Bewußtsein der Hörergemeinde sich einprägt, für wahr gehalten wird und so schließlich unaufhebbare Gültigkeit erlangt. Homer mußte also die Episoden für sein neues Achilleusbild sagenchronologisch gleichsam rückwärts erfinden und konnte in seiner Menis mit Rücksicht auf die Aithiopis besonders nicht nochmals – und anders! – den Tod des gleichen Achilleus erzählen.

8. Die Einfügung des Neuen ins Alte ist übrigens nicht ganz schlackenlos gelungen. Folgerichtig hätte Homer seinen Achilleus, der für den Kampf mit Memnon überleben mußte, zur

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[4] Pythia 6, 28ff.

[5] Daneben die vollen Formen Πατροκλέεος, -κλέεα, -κλεες, also ursprünglich etwa 'Des Vaters Ruhm habend', 'Berühmt wie sein Vater'. Aber Homer hat den Namen auf das mit κλέος verwandte κλυεῖν umbezogen, ähnlich wie den Namen der Εὐρύ-κλεια, die nicht 'Weithin berühmt' heißt, sondern 'Der man weithin gehorcht', vgl. υ 147ff). Diese Namen hat der Dichter eben nicht selber erst gebildet, sondern aus dem reichen schon bestehenden Namenschatz zu seinen poetischen Bedürfnissen passend ausgewählt und frei gedeutet.

[6] Denn Nestors Worte Λ 785–790, mit denen er Patroklos an die Mahnungen seines Vaters erinnert, auf die der Sohn zwar *nicht* hört, aber hören *sollte*, gehören nicht Homer, sondern einem Bearbeiter (vgl. Von der Mühll, *Kritisches Hypomnema zur Ilias* 200), der sich jedenfalls gerade vom Namen für das Motiv hat inspirieren lassen.

Rache an Hektor nicht in der Meinung ausziehen lassen dürfen, er werde diese Rache *sofort* mit dem eigenen Tod bezahlen müssen; denn das geschieht ja nun gar nicht sofort. Und doch läßt Homer die Mutter es ihrem Sohn so voraussagen, Σ 96: *αὐτίκα γάρ τοι ἔπειτα μετ' Ἕκτορα πότμος ἑτοῖμος* (vgl. schon 89ff und dann 115f). Aber Homer hat die leichte Inkonsequenz in Kauf genommen, um das auch für sein neues Achilleusbild unentbehrliche Motiv der Vorlage, Achills Wissen um den Tod als Preis der Rache (s.o. Abschnitt 3), nicht zu verlieren.

9. Nun haben aber gerade auch die Ereignisse der Aithiopis, die auf Achills Rache an Memnon folgten, Homer zur dichterischen Überbietung herausgefordert. Das war Achills Sturm auf die Stadt, sein Tod durch Apollon und Paris im Skäischen Tor, dann Kampf um die Leiche, Bergung, Aufbahrung, Totenklage durch Thetis mit ihrem Gefolge, Bestattung und Leichenspiele. Um auch diese großen Motive in seine Menisdichtung hereinzuholen, hat Homer mit einem genialen Kunstgriff sie alle vom Achilleus der Vorlage noch auf den von ihm erfundenen Patroklos übertragen. Dieser spiegelt also den Antilochos und den Achilleus der Aithiopis in einer Person.

10. Aber diese Verschmelzung ist, wenigstens an der Stelle, wo Patroklos fällt, nicht nur zum Vorteil der Dichtung ausgefallen. Homer hat nämlich seltsamerweise für den Tod seines Patroklos nicht etwa den Tod nur des einen der beiden Vorbilder nachgezeichnet, sondern die Tode des Achilleus und des Antilochos aus der Vorlage gleichsam aneinandergehängt. Das ergibt eine unbefriedigende Szene und verkleinert namentlich Hektors Heldenrolle. Denn bevor dieser den Patroklos erlegt, Π 818ff, gedichtet auf dem Hintergrund von Memnons Sieg über Antilochos, hat ihn schon Apollon gelähmt und entwaffnet und ein erst hier eingeführter Euphorbos in den Rücken geschossen, Π 785ff, gedichtet auf dem Hintergrund vom Tod Achills durch Apollon und Paris, so daß Hektor nun nur noch einem Wehrlosen den Rest gibt, wahrlich keine Ruhmestat. Es ist erstaunlich, daß Homer nicht schon selber an der Unvereinbarkeit der beiden Tode Anstoß genommen und den Ruhm, Patroklos besiegt zu haben, nicht ungeteilt seinem Hektor gelassen hat, den er doch sonst besonders liebevoll zeichnet. Erfand der Dichter unter einem inneren Zwang, von seiner Vorlage nichts fallen zu lassen, alle ihre Motive einzufangen und konkurrierend nachzubilden?

11. Wie dem auch sei, unvollkommen ist auch die Erfindung des bis dahin unbekannten Euphorbos für seine kurze Rolle. Ja, die Vorgeschichte des Mannes, Π 808–811, wird erst verständlich, wenn man weiß, wem er Zug um Zug nachgebildet ist. Und auch hier wieder hat der *Name* einen ersten Hinweis auf das Vorbild gegeben: Auf den Hirtennamen Εὔ-φορβος, 'Mit guter Weide', ist der Dichter beim Gedanken an die berühmte und folgenschwere Hirtenzeit des troianischen Königssohnes Paris im Idagebirge gekommen[7]. Übrigens zeigt gerade der Name Euphorbos, der die Aussetzung des Paris und damit das Parisurteil voraussetzt, zusammen mit weiteren, hier nicht zu erörternden Indizien, daß Homer auch andere Inhalte des epischen Kyklos kannte. Aber sein Gegenstand war der Held Achilleus, wie er ihm in der Aithiopis entgegentrat und ihn zur Zeichnung eines anderen, gewaltigeren Charakters herausforderte.

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[7] Ausführlich über Euphorbos in *Studi Micenei* 15 (1972) 79ff. – W. Schadewaldt, *Von Homers Welt und Werk* (1952²) 173, hat das doppelte Vorbild für Patroklos in Antilochos und Achilleus erkannt und in einem Schema veranschaulicht, aber noch ohne die Erfindung des Euphorbos zu verstehen.

12. Denn die Namen beweisen jetzt eben nicht nur, was die Motivanalyse zuvor schon wahrscheinlich gemacht hat, die Priorität der Aithiopis vor Homers Menis, sondern auch, daß Homers Achilleusdichtung, dieses Kernstück noch unserer Ilias, überhaupt erst im Ringen mit jener Vorlage konzipiert und durchgebildet worden ist. Wie frei Homers Genie dabei gegenüber seiner Inspirationsquelle blieb, erkennen wir besonders deutlich an dem, wozu ihn die Penthesileia-Episode der Aithiopis[8] angeregt hat. An Stelle der von fraulicher Wirklichkeit so weit entfernten Kriegerin und Anführerin zeichnete er, vermenschlichend nach seiner Art, ein diametral entgegengesetztes, dafür in seiner und in jeder Welt sehr reales Frauenbild: des Kriegers *Gattin* – Hektors Gattin. Von dem Vorbild – oder "Antivorbild" – steckt eine Spur noch in Andromaches taktischem Ratschlag an den Gatten, Z 433ff, den er sanft zurückweist, 490ff: "Walte du nur im Haus, der Krieg bleibt Sache der Männer, *πόλεμος δ'ἄνδρεσσι μελήσει*", im Sinne des Dichters, der die Amazone nicht mochte. Der Beweis aber von des Dichters Anregung durch die Penthesileia der Vorlage steckt, einmal mehr, im *Namen* der *Ἀνδρο-μάχη*. Dieser ist nicht nur nach seinem Sinn, 'Die gegen Männer Kämpfende', ein Amazonenname, sondern auch in der Literatur und auf Vasenbildern, und dort der bei weitem häufigste.

13. *Warum* Homer oft die Namen seiner Personen nicht auf diese selber, sondern auf deren Vorbilder hat hinweisen lassen, ist wohl schwer zu sagen. Immerhin stellen wir diese Art der Namengebung nicht nur bei ihm fest, sondern auch bei andern in unseren Homertext eingearbeiteten Epikern, z.B. beim Dichter der Ithakesierversammlung im *β*, der erst um 560 in Athen an der Odyssee weitergedichtet und in den Namen der dem Odysseus treuen Greise unter den Rednern auf den alten Solon, in denen der frechen Freier auf den Usurpator Peisistratos angespielt hat[9]. Das spricht dafür, daß diese seltsame Benennungsweise zur überlieferten epischen Techne gehörte. Ein Wille des Dichters, durch diskrete Hinweise den kundigeren seiner Hörer die Quellen und treibenden Kräfte seines Schaffens zu verraten oder seinen Vorgängern ein verschlüsseltes Denkmal zu setzen, ist kaum anzunehmen, zumal die antiken (und die modernen) Gelehrten solche Zeichen in den Namen gar nicht bemerkt zu haben scheinen.

14. Was auch immer einen Dichter zum Evozieren von Vorbildern in den Namen bewegen konnte, *daß* Homer und seine Erweiterer so gedichtet haben, ergibt für uns eine Forschungsmethode. Auf diesem Weg kann z.B. die vielerörterte Frage nach dem Verhältnis des Achilleuszornes zum Meleagroszorn entschieden werden, von dem der Dichter in Umbiegung älterer Sage den Phoinix im Paradeigma für Achilleus erzählen läßt, I 524–599: Die Rolle der *Κλεο-πάτρη*, 556ff, geht mit der des Patro-klos parallel, und die Namen sind aus den gleichen Elementen zusammengesetzt, also der eine aus dem andern durch Umstellung. Aber welcher ist des andern Vorbild? Nachdem klar ist, daß Patroklos nach der Heldentat des Antilochos benannt ist, kann nur Kleopatre nach Patroklos gebildet, der Meleagroszorn in Analogie zum Achilleuszorn erfunden sein[10].

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[8] Noch Pestalozzi S. 5 und Schadewaldt a.O. S. 158 glaubten, zu Unrecht, die Penthesileia-Episode sei "offensichtlich jünger", bzw. "sichtlich ein anderes Gedicht" als die Memnon-Episode. Die Behauptung, eine Liebe Achills zu Penthesileia könne erst lange nach Homer erfunden worden sein, ist willkürlich (richtig W. Kullmann, *Die Quellen der Ilias* 46f).

[9] *Studi Micenei* 17 (1976) 137–155.

[10] *Studi Micenei* 9 (1969) 86f und zuletzt *Živa Antika* 31 (1981) 87f.

15. Nach den Namen nun zu den Epitheta, und vorerst zu ἱπποκέλευθος (s.o. Abschn. 1.2.4ff). Denn auch dieses Wort verstehen wir erst, nachdem die Namen erwiesen haben, welche Szene der Aithiopis dem schaffenden Dichter vorschwebte, s.o. Abschn. 5. Es ist formal ein Possessivkompositum, 'für die Pferde einen Weg habend', auf Antilochos bezogen 'dem Wagen einen Ausweg findend', und hält so, in knappster Zusammenfassung, ein sehr besonderes, beispielloses und kaum wiederholbares Ereignis fest, einen Auftritt, für den allein es geschaffen worden ist, ohne Aussicht, je wieder in seiner richtigen Bedeutung Verwendung zu finden. Aber schon in seinem einzigen erhaltenen antiken Vorkommen, bei Homer in der Formel Πατρόκλεες ἱπποκέλευθε, Π 126.584.839, verliert es, auf das Abbild bezogen (s.o. Abschn. 6), allen Sinn. Welche Zumutung Homers an sein Publikum, das in seinem Gesang von Patroklos keine Episode zu hören bekam, wo dieser einen blockierten Wagen wieder in Fahrt gebracht hätte! Man konnte das Wort im Zusammenhang der Menis überhaupt nicht verstehen.

16. So ist es – mit einer noch aufzusparenden Ausnahme – auch für die Erklärer geblieben, antike und moderne. Ich zitiere Scholien, Übersetzungen, Lexika und Kommentare nicht einzeln. Sie erklären ἱπποκέλευθε als ἱππικέ; ταχύτατε; ἐφ' ἵππων ποιούμενε τὴν κέλευθον; παραπλησίαν ἵπποις ὁρμὴν ἔχων; 'qui combat sur un char; que transportent les chevaux; travelling by means of horses; driver of horses; rider; Rossetummler; einen langen Weg fahrend'; auch, mit falscher Ableitung von κελεύω, 'Pferden befehlend'. Auch wo das Wort frühbyzantinisch noch einmal in einem Gedicht erscheint, Anth. Pal. 9, 210, 9, steht es katachrestisch für 'Reiter'. Bentley fand das Wort zwar mit dem Element κέλευθος sinnlos im Zusammenhang, aber in der überlieferten Form auch nicht auf das ihm sinnvoller scheinende κελεύω zu beziehen – beides mit Recht – und wollte deshalb in *ἱπποκελευστά korrigieren[11]. Alle diese Deutungen tun der Wortbildung oder der Überlieferung oder dem Sinn von κέλευθος Gewalt an.

17. Aber nun findet sich unter den antiken Erklärungen doch auch einmal, schol. Π 126, die richtige: τοῖς ἵπποις εὐτρεπίζων ὁδόν, 'dem Gespann einen Weg bereitend', und das führt weiter. Hätte nämlich der Interpret, tüchtiger als alle seine Kollegen, das Wort *im Homer* richtig als Possessivkompositum erkannt, nur nach seiner Form und trotz seiner Sinnlosigkeit dort, so hätte er nicht εὐτρεπίζων, sondern ἔχων geschrieben. 'Einen Weg *bereitend*' dagegen erklärt ausdrücklich das Verhalten des Antilochos, und dieses kann der Interpret bei der Einmaligkeit jener Tat[12] letztlich nur aus der Aithiopis gekannt haben, sei es direkt aus dem noch erhaltenen Epos, das Vergil noch gelesen zu haben scheint, sei es aus Glossarien, in welche die Deutung des so ungewöhnlichen Wortes aus einem Kommentar

[11] F. Bechtel im *Lexilogus zu Homer* s.v. hat ἱπποκέλευθος als poetische Bildung erkannt und bekennt die Unverständlichkeit des Ausdrucks. Auch D. Page, *History and the Homeric Iliad* (1959) 256 und 286 Anm. 90, hat die Unvereinbarkeit des Epithetons mit der Rolle des Patroklos notiert, aber nicht mit dem Vorbild aus der Aithiopis und der es verratenden Aussage der Namen zusammengesehen, so daß er, zu Unrecht, noch an einen vorhomerischen Patroklos denken konnte, der "had much to do with horses"; er glaubte, das Wort gehöre zu κελεύω.

[12] Denn das, was Apollon O 260f dem Hektor verspricht, ἵπποισι κέλευθον πᾶσαν λειανέω, also mit den Elementen des Wortes ἱπποκέλευθος, und 355–366 ausführt, indem er den Graben des Achäerlagers zuschüttet und den Wall schleift, um den Wagen eine Bahn zu öffnen, ist doch etwas sehr anderes, als was Antilochos am Wagen seines Vaters tat, um ihn zu deblockieren. Übrigens ist Apollons Einsatz im O mit starken Gründen einem Bearbeiter zugeschrieben worden, vgl. Von der Mühll, a.O. 230.

zur Aithiopis eingegangen war. So oder so, das Wort hat in der Aithiopis gestanden, Homer hat es also nicht etwa selber im Gedanken an Antilochos erst neu gebildet[13], sondern fertig aus der Vorlage übernommen und ohne Rücksicht auf seinen Sinn auf Patroklos übertragen.

18. Dieser punktuelle Befund erlaubt Schlüsse sowohl auf die Aithiopis als auch auf Homers Art zu dichten. Man kann abschätzen, wo in der Aithiopis das Wort ἱπποκέλευθος gestanden haben muß. Die Möglichkeiten lassen sich nämlich einschränken, zunächst metrisch: Als Epitheton des Antilochos stand das Wort vor oder nach diesem Namen und mit diesem zusammen in einem der fünf Casus, aber von den zehn Kombinationen ist im Hexameter eine einzige leicht einsetzbar, der Vokativ in der Abfolge Name – Beiwort am Versanfang[14]. Damit ist der eingangs formulierte Halbvers, Ἀντίλοχ' ἱπποκέλευθε, gegeben. Auch ist sein Ort in der Erzählung nun eng eingegrenzt; denn so anzureden war der Held erst nach seiner Rettertat und eigentlich nur noch durch seinen Besieger Memnon, kurz vor oder kurz nach ihrem Kampf, vgl. Π 830 bzw. 859. Im Unmut über den unerwarteten Verlust eines scheinbar sicheren Sieges über den berühmten Nestor macht Memnon das, was er soeben als unerhört kühne Tat mitangesehen hat, zum eigentlichen Wesen des Jünglings, der sich ihm unvermutet und ohne alle Aussicht, ihm gewachsen zu sein, in den Weg stellt, und faßt dessen Tat, extrem verdichtend, in das einzigartig gebildete Wort – "du Streitwagendeblockierer!" Ein so kunstvolles Epitheton hat nur ein hochbegabter Dichter prägen können. Schon er hat offenbar ein bedeutendes Epos geschaffen. Sonst hätte kaum ein Homer sich davon zur Überbietung reizen lassen.

19. Nun also zu Homer. Wir haben aus seiner seltsamen Art, mit dem Namen des Patroklos auf sein Vorbild für diesen, auf den Antilochos der Aithiopis, hinzuweisen, verallgemeinernd eine Methode gewonnen, um von Namen homerischer Personen auf ihre Vorlage zu schließen, z.B. von Euphorbos auf Paris, von Kleopatre auf Patroklos, von Andromache auf Penthesileia, natürlich immer nur im Zusammenspiel mit andern Indizien. Wir werden nun aus Homers noch seltsamerer Art, das Epitheton ἱπποκέλευθος von jenem Antilochos sachlich unzutreffend auf dessen Abbild Patroklos zu übertragen, wieder verallgemeinernd noch eine andere Methode gewinnen, die dem gleichen Ziel dient. Denn jenes Epitheton ist nicht ein vereinzelter Fall. Vorerst ein Beispiel aus der Odyssee:

20. Homer nennt die Nymphe Kalypso 'tückisch', δολόεσσα (η 245), was sie, die liebende, verstehende, verzichtende, mit keiner Faser ihres Wesens ist. Das Epitheton hat Homer, auch hier wieder trotz dessen unpassender Bedeutung, auf seine Neuschöpfung übertragen von der Gestalt, die ihm für die Erfindung der Nymphe eine Vorlage hergab, nämlich von der Kirke, die so bezeichnet wird (ι 32) und so war, wenn sie die Ankömmlinge in Schweine verwandelte. Nicht viel besser paßt auf Kalypso in der nächsten Zeile (η 246) δεινὴ θεός, 'schreckliche Göttin', nach dem Formelvers für Kirke (κ 136 = λ 8 = μ 150). Die andern Indizien für dieses Verhältnis der beiden Frauengestalten, aus deren göttlicher Liebe Odysseus weg- und zu Penelope zurückstrebte, hat U. v. Wilamowitz schon vor hundert Jah-

[13] Ich korrigiere hiermit meine in *Studi Micenei* 9, 91 geäußerte frühere Ansicht.

[14] Nicht wohl am Versende; denn die vor dem Vokativ dann anzunehmende Zäsur fiele in eine dafür nicht beliebte Stelle und würde den Vers halbieren.

ren notiert, *Homerische Unters.* 115ff, nicht widerlegt durch H. Güntert, *Kalypso* (1919)[15]. Wir erkennen in Kalypso gegenüber Kirke wieder, wie in Andromache gegenüber Penthesileia, Homers Willen und Kunst, das Unwirkliche und den Zauber durch ein menschlich Wahres und Wesentliches zu ersetzen und zu überbieten.

21. Wir haben an den Epitheta *ἱπποκέλευθος*, *δολόεσσα*, *δεινή* die Entlehnung vorerst deshalb beachtet, weil diese Wörter im neuen Kontext nicht oder schlecht passen. Aber Homers eigenartige Praxis, Epitheta von Vorbildern auf seine Neuschöpfungen zu übertragen, war natürlich nicht auf solche beschränkt, die dann nicht paßten. Im Gegenteil, die meisten so entlehnten blieben gewiß auch im neuen Zusammenhang sinnvoll oder wenigstens nicht sinnwidrig. Um auch solche festzustellen, achten wir also auf andere Indizien als nur auf ihre Unangemessenheit. Es kann z.B., umgekehrt, ein Epitheton zwar im neuen Kontext nicht sinnlos, aber auf das Vorbild ganz besonders zugeschnitten sein. Oder es ist bei Homer ausschließlich (oder fast ausschließlich) einer einzigen Person beigegeben, obwohl es zu andern Personen ebenso gut passen würde. Oder es ist als Beiwort auch des Vorbildes bezeugt, direkt oder indirekt. Die Kumulierung mehrerer dieser Indizien bei einem einzelnen Epitheton führt dann zur Gewißheit, zumal wenn mehrere Epitheta eines Vorbilds auf sein Abbild übertragen sind.

22. Man wird beachten, daß schon bei den drei hiervor besprochenen Wörtern je mehrere dieser Indizien die Entlehnung verraten. Wir suchen jetzt solche entlehnten Epitheta unter denen Hektors, den Homer in seiner Menis als Sieger über Achills Freund und Opfer von Achills Rache aus der Rolle Memnons in der Aithiopis heraus erfunden hat, s.o. Abschn. 4. Von den zahlreichen Beiwörtern Hektors bespreche ich hier drei, die sich mit Sicherheit als Entlehnungen von der Vorlage erweisen.

23. *χαλκοκορυστής* 'erzbehelmt, erzbewaffnet' ist achtmal Epitheton Hektors, einmal (Z 199) Sarpedons. Auch der ist ein Abbild Memnons; denn Homer hat für beide konstitutiven Elemente seines Patroklos je den gemäß der Aithiopis zugehörigen Gegner eingeführt, für das antilochische den Hektor, der ihn besiegt wie Memnon den Antilochos, für das achilleische den Sarpedon, den er besiegt wie Achill den Memnon. Die Beschränkung von *χαλκοκορυστής* auf diese beiden Helden, obwohl auch die andern erzbehelmt sind, ist ein erstes Indiz für die Übertragung des Wortes vom Memnon der Aithiopis auf Hektor und Sarpedon. Hinzu kommt, zweitens, daß es bei Memnon besonders angezeigt scheinen mußte, in einem Epitheton seine Rüstung zu evozieren; denn er kam, wie noch der Auszug aus der Aithiopis bei Proklos es festhält, *ἔχων ἠφαιστότευκτον πανοπλίαν* den Troianern zu Hilfe. Die Übertragung wird zur Gewißheit, wenn, drittens, in Hesiods Theogonie 984, letztlich aus der gleichen Quelle, Memnon selber das Beiwort erhält: *Τιθωνῷ δ' Ἠὼς τέκε Μέμνονα χαλκοκορυστήν.*

24. *κορυθαίολος* 'helmblitzend'[16] ist achtunddreißigmal Beiwort Hektors, ein einziges Mal nicht, Y 38 von Ares in der erst nachhomerischen Theomachie. Bei Homer gehört es also

[15] Aber v. Wilamowitz schloß daraus zu einfach (116), daß "es folglich eine Zeit gab, wo Odysseus zwar bei Kirke war, aber nicht bei Kalypso." Welcher Held bei Kirke geweilt hat in jener (korinthischen?) Quelle, aus welcher ein späterer Bearbeiter (der letzte?) die Zauberin zum Glück noch in unsere Odyssee hereingeholt hat, bleibt ungewiß (Iason?).

[16] Nicht 'helmschüttelnd'; s. die Diskussion bei Page, a.O. 249f und 288f Anm. 92–95.

nur dem Hektor. Diese Ausschließlichkeit und die Bedeutung des Wortes, das wie χαλκοκορυστής zu den Hephaistoswaffen Memnons paßt, zeigen deutlich, daß auch dieses Epitheton von Memnon auf Hektor übertragen ist, zumal es mit den andern Übertragungen von Memnon auf Hektor und von Antilochos auf Patroklos parallel geht. Bei Memnon stand es freilich noch nicht wie bei Hektor in einer Versschlußformel[17].

25. ἀνδροφόνος 'männermordend' ist elfmal Beiwort Hektors, einmal des Ares, einmal des Lykurgos, und dreimal bezeichnet es die Hände Achills. Die Bedeutung paßt zu jedem Krieger. Umso auffälliger ist die Häufung des Epithetons bei Hektor unter Ausschluß so vieler andern. Das würde kaum genügen, um das Wort für Hektors Vorbild zu beanspruchen, käme nicht hinzu, daß Pindar in seinen Versen über des Antilochos Tat, Pythia 6,30, für Memnon das mit ἀνδροφόνος synonyme ἐναρίμβροτος passend gefunden hat, evident als Paraphrase von Formeln wie *Μέμνονος ἀνδροφόνοιο aus der Aithiopis[18].

26. Zurück zu Patroklos. Neben dreimaligem Πατρόκλεες ἱπποκέλευθε (s.o. Abschn. 15) steht konkurrierend in der Patroklie viermal Πατρόκλεες ἱππεῦ[19]. Homer braucht ἱππεύς sonst nur im Plural, nie als Epitheton. Als solches stand es zweifellos in der Aithiopis bei Antilochos, dem Vorbild, etwa in einer Versschlußformel *Ἀντίλοχ' ἱππεῦ. Zu diesem paßte es besonders gut, weil gerade Nestor und seine Söhne als Streitwagenkrieger und auch sonst als Fahrer berühmt waren.

27. Denn so viel von pylischer Tradition ist noch faßbar. Daraus schöpft der letzte Bearbeiter Homers, in Athen im Dienst und zu Ehren der Peisistratiden, wenn er Nestor im Λ, 720–761, mit seinen Jugendtaten als Wagenkämpfer prahlen, wenn er ihn im Ψ, 304–348, seinem Antilochos Anweisungen für das Wagenrennen geben, wenn er ihn im Λ, 510ff, mit seinem Wagen den verwundeten Machaon aus der Schlacht retten, auch wenn er, γ 482ff, den andern Nestorsohn Peisistratos sich im Fahren auszeichnen läßt. Aus der gleichen Tradition schöpfte schon lange zuvor in Ionien, wo man sich ebenfalls auf pylische Herkunft berief[20], der Aithiopisdichter, wenn er den Nestoriden Antilochos in der Streitwagenbedrängnis seines Vaters sein Heldentum beweisen ließ. Der Γερήνιος ἱππότα Νέστωρ ist eben in mindestens zwei Malen in die troianische Epik aufgenommen worden, einmal vor und einmal nach Homer, und was für den Vater ἱππότα, das war für den Sohn ἱππεύς. Nicht zufällig galt der Gott der Pferde, Poseidon, als Ahn der Neleiden, des pylischen Geschlechts, aus dem auch die Peisistratiden sich herleiteten[21].

28. Die hier vor allem am Verhältnis von Homers Menis zur Aithiopis dargestellte seltsame Art, mit Personennamen Inspirationsquellen zu evozieren und Epitheta von Vorbildern

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[17] Auf Memnon und seine Hephaistoswaffen geht vielleicht auch Hektors Beiwort φαίδιμος zurück (29mal). Der Glanz der von Hephaistos geschmiedeten Rüstung ist bei Homer ein unterscheidendes Merkmal auch Achills, wenn Priamos diesen daran erkennt und der Dichter diesen Glanz mit dem des Sirius vergleicht, X 24–32. Aber φαίδιμος steht vierzehnmal auch bei andern Helden.

[18] Ähnlich hat Bakchylides ein überliefertes Beiwort durch ein Synonym ersetzt, wenn er die in ätolischer Epik ursprüngliche Formel ἱππηλάτα Οἰνεύς (I 581, vgl. ἱππηλάτα Τυδεύς Δ 387) für seine Daktyloepitriten in πλάξιππος Οἰνεύς abwandelte, s. *Studi Micenei* 9,85; *Živa Antika* 31 (1981) 89 Anm. 12.

[19] Π 20. 744. 812. 843.

[20] Mimnermos 12 D.

[21] Herodot 5, 65, 3.

mechanisch auf Neuschöpfungen zu übertragen, zeigt Homer nicht nur in der Ilias. In der Odyssee, s. oben Abschn. 20, überträgt er Epitheta von Kirke auf Kalypso. Der Name des Πολύ-φημος, Synonym der Hadesepiklese Klymenos, verrät, woran Homer bei der Erfindung dieses Poseidonsohnes dachte[22]. Der 'göttliche Sauhirt' Eumaios hat seinen Namen nach dem quasi-synonymen des wohlwollenden Unterweltsgottes Eubuleus, der nach der Legende ebenfalls Schweinehirt und Helfer der Demeter war[23] und dem Dichter den treuen Diener des Odysseus eingegeben hat. Auf die gar originell wirkende Verbindung δῖος ὑφορβός (auch δῖ' Εὔμαιε) kam Homer, weil er den Gott im Kult als *Zeus* Eubuleus kannte, und zum Vater gab er dem Eumaios einen Κτήσιος (*o* 414) nach dem ebenfalls unterweltlichen Zeus Ktesios[24].

29. Die sonderbare Wahl von Namen und Übertragung von Epitheta beschränkt sich auch nicht auf Homer. Die Redner der Ithakesierversammlung im β (s.o. Abschn. 13) gehören einem viel späteren Dichter, die Benennung des Nestoriden Peisistratos in γδο zu Ehren der Familie des Tyrannen erst dem letzten Bearbeiter. Umgekehrt hat schon vor Homer der Dichter der Ur-Argonautika die Phäakenkönigin Ἀρήτη wegen ihrer Rolle als Ehestifterin zwischen Iason und Medeia nach Hera, der Göttin der Ehe, erfunden, ihr, weil Medeia sie anfleht, diesen Namen 'Zu der man fleht' gegeben und noch dazu das stehende Beiwort der Hera, λευκώλενος, auf sie übertragen. So ist sie in unsere Odyssee (ζ 303–315, η 53ff) und, auf andern Wegen, in das Epos des Apollonios eingegangen[25].

30. Ebenfalls vor Homer, aber als schon feststand, daß Achilleus höchsten Kriegsruhm erntet und fern der Heimat einen frühen Tod findet im Dienst anderer, denen sein Verlust schwere Niederlagen bringt, und als sein Name längst auf ἄχος bezogen wurde und sein Charakterbild mitbestimmte, hat ein Dichter, spätestens 'Arktinos', in der Amazone ein weibliches Gegenstück zu Achilleus geschaffen und mit Blick auf diesen benannt[26]; die genaue Entsprechung zu Ἀχι-λ(λ)-εύς, mit πένθος für ἄχος, *Πενθ-ίλ(λ)-εια, metrisch unbrauchbar bzw. unbequem, hat er nach Πρωτεσίλαος, Ἀρκεσίλαος, ἐλκεσίπεπλος u.a. erweitert zu Πενθεσίλεια.

31. Es haben also schon Homers Vorläufer, wie danach er selber und seine Bearbeiter, in den Namen für neu erfundene Personen auf deren Vorbilder Bezug nehmen können. Sich von solchen Gedankenassoziationen bestimmen zu lassen, gehörte offenbar zur Schaffensweise der frühen Dichter-Sänger überhaupt, und ebenso gewiß auch das Übernehmen von Epitheta der Vorlage, unbesehen, ob sie zur neuen Person paßten. Es erklärt sich am ehesten aus der jahrhundertelangen *Mündlichkeit* des vorgeschichtlichen Heldenliedes. Der Übergang zur Schriftlichkeit ist ja nicht plötzlich erfolgt und war nie total, solange Sänger auswendig vortrugen. Die beiden hier erörterten Besonderheiten epischen Erfindens, von der doch intensiv betriebenen *oral-poetry*-Forschung unseres Jahrhunderts m.W. nicht erkannt, gewähren jedenfalls unerwartete und sichere Einsichten in die Schaffensweise der alten Epiker und lassen die Überlegenheit Homers sichtbar werden.

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[22] *Antike und Abendland* 25 (1979) 141–147.
[23] Pausanias 1, 14, 3.
[24] *Antike und Abendland* 30 (1984) 148–153.
[25] *Antike und Abendland* 25 (1979) 160ff.
[26] Ein junger Mitforscher, David Bouvier, kommt unabhängig, auf Grund trefflicher Einzelbeobachtungen, zum gleichen Schluß (briefliche Mitteilung).

Initial PIE *g^wh- in Germanic

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The problem of the labiovelars and their reflexes in Germanic has recently been the object of renewed interest in connection with the controversial hypothesis of Elmer Seebold (1967; 1980) that the Indo-European labiovelar aspirate and the clusters **ghw* and **ĝhw* (i.e. 'velar' or 'palatal' + -w-) become *b-* initially in Germanic, except before *u* where they are reflected by *g-*. Reviewing the evidence for labialization of labiovelars in Germanic, Thomas Markey (1980) posits a rule establishing a major isogloss between East Germanic and Northwest Germanic and accounting for the labialization of back spirants before resonants, e.g. Go. *bagms* versus OHG *boum* 'Tree'. For initial **g^wh-*, Markey focuses on Gmc. **warma-* 'warm' which he derives from IE **g^whor-mo-*, but he sees a reflex of IE **$g^wh_{\circ}r$-mo-* in ON *Garmr*, the name of the 'hound of Hell', which also appears in a kenning for 'fire' (*elris garmr*). He accounts for NGmc. **garm-* as follows:

IE **$g^wh_{\circ}r$-mo-*
↓
PGmc. **gurma-* (delabialization before *u* from **$r_{\circ}$*);
↓
Gmc. **gorma-* (lowering of *u* to *o* by *a*-Umlaut);
↓
ON **garm-* (*late* Gmc. coalescence of *o* and *a*).

The existence of such pairs as ON *gandr* vs. Da.dial. *vann* (OE *wenn*) < **$g^whon(yo)$-* which suggests "an apparently arbitrary alternation of *g-/w-* as reflexes initially before *a*" (Markey 1980: 287), would then imply similar diachronic explanations.

In a much more ambitious monograph on the Proto-Indo-European labiovelars A.G.E. Speirs (1978) takes the occurrence of 'irregular' labials such as the initial *b-* of Latin *bos* 'ox, cow' or the medial *-f-* in Gmc. **wulfa-* 'wolf' as an excuse to question the whole traditional view about the treatment of the labiovelars in the Indo-European dialects. Listing the apparent alternations between labial and non-labial reflexes of Indo-European gutturals (e.g. Latin *haedus*: *fēdus* [Sabellic] 'young goat'; *faba*: *haba* [Faliscan] 'bean'; OHG *krioh-han*: OE *crēopan* 'creep'; Go. *leihwan* 'lend': OHG *bi-līban* 'remain, be left'; etc.), Speirs elaborates a theory ascribing most of these alternations as well as the 'Reimwortbildungen' of the same kind, described by H. Güntert (1914), to early Indo-European labialization and dentalization processes, similar to those evidenced by Greek *theínō* 'strike, kill': *phónos* 'murder' < PIE **g^when/g^whon-*. Accordingly, in his system **k^w* would become **t* before front vowel and **p* before back vowel, being, however, delabialized to **k* before *u* or as a result of dissimilation processes. Thus, a Proto-Indo-European root **k^wew-* would appear as **tew-* ~**pow-* ~**ku-* under the conditions of the regular 'ablaut' alternations, but with

paradigmatic leveling and restructuring, this pattern would be likely to be reflected by the three types: (1) *tew- ~tow- ~tu-*; (2) *pew- ~pow- ~pu-*; (3) *kew- ~kow- ~ku-* in the dialects (Speirs 1980: 69). With such a hypothesis, Speirs is obviously able to account for the most unexpected 'correspondences': since PIE **g^{w}ew-g^{w}(h)* 'bend' (Speirs 1980: 28) can be reflected by **bhew-g-, *dhew-g(h)-, *dhew-b(h)-*, etc., it can serve as the etymon of E *bow, duck, dip*, to which – with the basic meaning of convexity – Speirs (1980: 31) adds E *cudgel, pock, cob* ('round lump') and even *pudding*! As for cases where labiovelars really do appear, e.g., Go. *qiman* versus OE *cuman* 'come', Speirs (1980: 69) states: "The labiovelar was no doubt restored in the third group [i.e. **kew- ~ *kow- ~ *ku-*] before the paradigm was irrevocably split."

It stands to reason that little credence can be given to a hypothesis that endeavors to derive from a same root **g^{w}her-* in the expanded form **g^{w}hr-em-* Gk. *chremízō* 'neigh, whinny', OCS *grĭměti* 'thunder, roar', Skt. *bhramara-* '(bumble) bee', OHG *bremo* 'gad-fly', OE *drēam* 'joy, rejoicing, music, song', because they all express various sounds, and then proceeds to do the same with **g^{w}r-em-* (Gk. *brémō* 'roar, clamor'), **g^{w}hel-* (allegedly in Gmc. **galan-* 'sing, call' and **baljan-* [OHG *bellan*] 'bellow, bark'), **g^{w}el-* (allegedly in ON *kalla* 'call, shout, say, tell' and Lith. *bylóti* 'say, speak').[1] The gratuitous assumption of extensive leveling and analogical restructuring is perhaps, beside the rather superficial semantic analysis, the greatest flaw of this extremely speculative new approach to the labiovelars.

The views of T. Markey are much closer to the traditional theory, whose major points he usefully summarizes (1980: 287–288):

(1) Labiovelars never appear before *u* in the *centum* languages, where the labial element is obviously redundant.
(2) Delabialization appears to be more extensive in peripheral *centum* languages like Old Irish or Tocharian, but in Old Irish, at least, it must be a late development (post-Ogamic, according to Markey, on account of the *u* of OIr. *cruth* 'shape' versus W *pryd* 'time' < **k^{w}ṛtu-*).
(3) The labial element does not appear in Germanic before Gmc. *u*, stops or resonants as well as in final position.
(4) Labiovelars and clusters of velar + *w* are merged in Proto-Germanic and show the same reflexes: /k^{w}/, /x^{w} ~h^{w}/, /g^{w} ~ɣ^{w}/.[2]
(5) Intervocally, Gmc. /g^{w} ~ɣ^{w}/ appears to have been dissimilated to *-g-* before back vowels (especially *u*) and to *-w-* before front vowels, e.g. Go. *magus* 'boy' < **maɣ^{w}ú-*: **maɣ^{w}ī́* > *mawi* 'girl'. Analogical leveling has however distorted the original pattern, especially in verbal paradigms.

As to initial IE **g^{w}h-*, Markey (1980: 290) resolutely rejects the labial thesis of Seebold as being based on specious etymologies like the connection of Go. *bidjan* with Gk. *théssamai* 'pray'. For the etyma where IE **k^{w}* is reflected dialectally by *f*, Markey (1980: 291) points out that the complex dissimilation/assimilation processes involved in this residual group of etyma (Go. *fidwor* '4', *twalif* '12'; OFri. *fiāl* 'wheel'; ON *lifr* 'liver', etc.) "represent a stage of late, dialect specific transformations of the labiovelars inherited by Germanic". It is in the same perspective that he then tries to solve the problem of the aberrant cases with Gmc. *g-* before *-a-* as reflex of IE **g^{w}h-*. While he agrees basically with the traditional rules:

(a) IE *$g^{w}h$- before IE *o is reflected by *w- in Gmc., e.g. OE *wearm*: Skt. *gharmáḥ* 'warm';

(b) IE *$g^{w}h$- before Gmc. *u is reflected by *g- in Gmc., e.g. ON *gunnr*, OE *gūþ* 'battle' (< Gmc. **gunþa*-), Markey reorders the rules of a-Umlaut and lowering of o to a in North-Germanic. This is precisely what makes his explanation of NGmc. **garm*- inacceptable, because, if we applied the rules as implied by his hypothesis, the diachronic development of PGmc. **þurpa*- would yield **þarp* instead of *þorp* 'farmhouse, village' in Old Norse, according to the scheme:

PGmc. **þurpa*- (with *ur* from IE **r̥*)
↓
Gmc. **þorpa*- (a-Umlaut)
↓
NGmc. **þarp(a)*- (lowering of o to a).

It is, therefore, obvious that Markey's explanation of ON *garmr* as a reflex of IE *$g^{w}hr̥mo$- has to be rejected and can, accordingly, not serve as a model for the explanation of alternations like ON *gandr* 'wand' vs. Da. dial. *vann* 'wen'.[3]

Markey (1980: 286), while discussing the views on the primordial existence Proto-Indo-European labiovelars, clearly leans towards the hypothesis advocated by Jerzy Kuryłowicz (1935: 1–26; 1956: 356–366) deriving the labiovelars from the velars through secondary – conditioned – labialization. This hypothesis is analyzed in considerable detail by Lars Steensland (1973), who reaches a different conclusion after a thorough review of the available etymological material. He concludes (1973: 117) that the oldest system of Proto-Indo-European must have had two 'gutturals' with the following realizations:

/kA/ : [$k^{(+)}$e, k^{+}i, ko, ka]
/kB/ : [k^{w}e, k^{w}i, k^{w}o, k^{w}a] [ku]

– but that neutralization before /o/ and /a/ occurred at a very early date, reducing the system to:

/kA/ : [$k^{(+)}$e, k^{+}i]
/kB/ : [k^{w}e, k^{w}i] [ko, ka, ku]

/kA/ is assumed to have a palatal articulation [k^{+}], which led to its palatalization to [k']; morphological leveling spread this [k'] before back vowels, especially o, less u, seldom a – hence, secondarily [k'o, k'u, k'a] for [ko, ka, ku].

/kB/ was labialized, but with early neutralization of the labialization before [o] and [a] the unlabialized forms were likely to spread before e, especially in roots with the ablaut *eu*~*ou*~*u* – hence, secondary ($k^{(+)}$e) for [k^{w}e].[4]

Steensland does not deal specifically with the reflexes of labiovelars in Germanic, but if he is right in assuming that the series [k^{w}e, k^{w}i, ko, ku, ka] represent the "Ausgangspunkt für die Gutturalsysteme aller urindogermanischen Sprachen" (1973: 117), it would imply that what we reconstruct as *$g^{w}ho$-, alternating with *$g^{w}he$-, would actually already be delabialized to [gho] in Pre-Germanic and accordingly be reflected by initial [ɣa] in Proto-Germanic. Hence, ON *geð* (< PGmc. *ɣaðja-; de Vries 1961: 159) 'mind, mood, spirits' would closely correspond to OIr. *guide* 'prayer' (< IE *$g^{w}hodhyā$; Pokorny 1959: 488) and ON *gǫrr*, OHG *garo*, OE *gearu* (< PGmc. *ɣarwa-) 'ready' (lit. 'cooked' – cf. Du. *gaar*, G. *gar*) could be derived straightly from PIE *$g^{w}hor$-*wo*-, parallel to *$g^{w}hor$-*no*- in Lat.

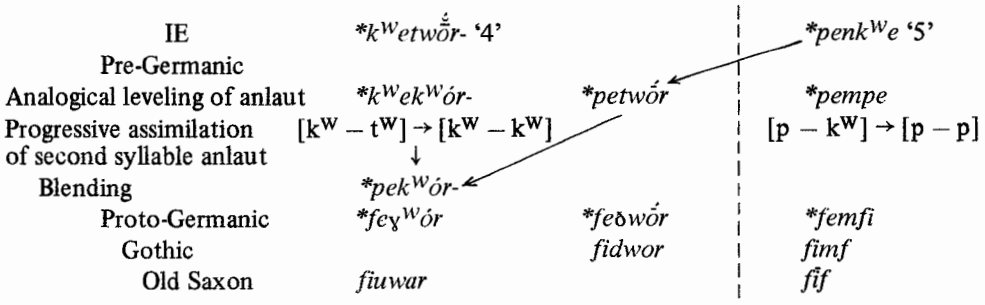
fornus 'oven', OIr. *gorn* 'fire' (Pokorny 1959: 494). But Steensland has obviously not made up his mind about the reflexes of initial PIE **g^{w}h-* in Germanic, since he refers to Seebold's rejection (1967: 112) of the above-mentioned etymology of ON *geð* and labels the derivation of ON *varmr* from IE **g^{w}hormo-* "umstritten" (1973: 14). However, he definitely assumes that initial *g-* corresponds to PIE **g^{w}h-* in OE *gūþ* (*gȳþ* in compounds) 'combat' < PIE **g^{w}hn̥tyā*, from the root **g^{w}hen-(ə-)* 'hit' (1973: 27).

This brief review of the recent discussion of the problem of the reflexes of initial **g^{w}h-* in Germanic indicates that, while apparently no one wants to follow Seebold, no satisfactory solution has yet been given either.

Summarizing the debate, it is, however, possible to make the following points:

(1) Wherever labials appear to reflect labiovelars in Germanic, each case has to be explained individually in its own dialectal and phonological context, as George Lane (1939: 192) already indicated:

(a) with the numerals for 'four' and 'five' we are obviously dealing with assimilation and analogical leveling processes, which could be tabulated as follows:



(b) in the case of ON *ofn*, OHG *ovan*, OE *ofen* 'oven, furnace' versus Go. *auhns*, OSwed. *ugn*, the possibility of a Celtic loan alongside the inherited term is to be considered in view of the more advanced metallurgic technology of the Celts (Birkhan 1970: 142–147);

(c) in the case of the 'wolf' the occurrence of *-p-* forms in both Germanic (Go. *wulfs*) and Italic (Lat. *lupus*) suggests a Pre-Germanic blending of IE **wĺ̥kʷos* (metathetized **lúkʷos*) with the root **wlp-* of Lat. *volpēs* 'fox', Lith. *vilpišỹs* 'wildcat' (Lane, 1939: 194),[5] perhaps ascribable to tabuization.

(2) The etymologies advanced by Seebold (1980: 477–484) to assume the labialization of initial PIE **g^{w}h-* in Germanic are unconvincing:

(a) ON *bani*, OE *bana*, OHG *bana* 'killer' would undoubtedly fit semantically under PIE **g^{w}hen-* 'hit, slay', but the connection with Av. *banta-* 'ill' is unimpeachable: illness is due to the action of evil spirits in Zoroastrian belief; they strike the victim like the killer inflicts a mortal wound (OS *beni-wunda*). From the same root **bhen-* 'hit, injure' (Pokorny 1959: 126) Germanic also derives MHG *bane* (G *Bahn*), MDu. *bane* (Du. *baan*) 'open space, road', which points to an original meaning 'beaten ground, flattened area' (de Vries 1971: 24).

(b) Go. *brinnan*, ON *brinna*, OE *beornan*, OHG *brinnan* 'burn', allegedly a *-nu*-present to **g^{w}her-* 'burn' (**g^{w}hr̥-nw-*; cf. Skt. *ghr̥ṇóti* 'glows, gleams', Arm. *jernum* 'I heat, burn') with an analogical full grade vowel – Gmc. **brenn-* instead of *brunn-* –

has been connected with the theme (II) **bhr-ew-* of the root **bher-* 'gush' or 'boil up' (of water), 'flare up' (of fire) (Pokorny 1959: 132–133, 143–145), expressing the concept of 'welling forth' in Gmc. **brunō, *brun(e)n-* 'spring' (Go. *brunna*, OE *burna*, OHG *brunno*) and the concept of 'leaping up flames' in Gmc. **brinnan-* (with its causative **brannjan-* in Go. *brannjan*, ON *brenna*, OE *bærna*, OHG *brennan*). Rather than the infixation of a nasal in the theme (II) **bhr-ew-*, it might, however, be preferable to assume a theme (II) **bhr-en-*, followed by a present tense suffix *-w-*, expanded over the whole paradigm (hence *-nn-* < **-nw-*) and the derivatives (van Coetsem 1972: 193): this would account for the *-n-* ~*-nn-* alternation in forms like ON *bruni* 'burning, fire': *brenna* 'burn' and be paralleled by the theme **bhr-em-* in OInd. (RV) *bhramáḥ* 'whirling flame', *bhŕ̥miḥ* 'restless, active' (applying to Agni, RV I 31.16). As for the semantic development ('flare up' → 'burn'), the connotation 'licking flames' is further substantiated by related terms such as the Vedic intensive *járbhurīti* or MIr. *breo* (< **bhriwo-*) 'flame'.

(c) The derivation of Go. *bidjan*, ON *biðja*, OE *biddan*, OHG *bitten* 'ask, entreat, demand' from **g^{w}hedh-* 'ask, demand' (Pokorny 1959: 488) is described as "an sich bestechend" by Wolfgang Krause (1968: 123–124). Seebold's comments on the use of Ved. *bādh-* (1980: 480–481) are, however, extremely lopsided, focusing only on a specific use of the term in the Vedic compound *jñu-bādh-* describing a devotional genuflection: pointing out that the corresponding use of OS *an kneobeda* and its parallels (OE *on cneowgebedum*, ON *á knébeðu*) reflects an effort to translate into Germanic this Oriental practice, he concludes rather hastily that "die angeführte etymologische Gleichung nur einen Sinn hat wenn das germanische Wort 'Knie*beuge*' bedeutete" (1980: 482). This is, however, disregarding the extensive use of RV *bādh-* in other contexts where it essentially seems to mean 'to urge', e.g. in RV I 61,2, where Geldner (1951: 1.78) considers *bā́dhe* as an infinitive which he translates tentatively as 'nötigen' (a song of praise is offered to Indra to soothe and *impel* him); similarly, in RV I 132, 5, *bā́dhe* refers to efforts to 'overwhelm' the deity with praise; in RV VI 11, 1, the sacrificing priest is called upon to act as 'urged on' by the Maruts (Geldner 1951: 2.103); etc. This concept of 'pressing, forcing' tallies with the meanings 'order, require' appearing in early southern Germanic sources, e.g., OHG *pitit.* poscit (Ahd. Gl. I. 230. 14), OE *ic bidde.* peto (AElfric), beside 'pray, request, entreat', and as Alfred Goetze (1939: 344) rightly points out: "*bitten* kann dauernd den verschiedenen Haltungen von 'befehlen' bis 'flehen' gerecht werden, je nach dem gesellschaftlichen Verhältnis des Sprechenden zum Angesprochenen". Since PIE **bhedh-* (Pokorny 1959: 114) is formally unimpeachable and semantically quite acceptable as the source of Gmc. **biðjan*, there is no need to look elsewhere for etymological linkage.

The other etymologies adduced by Seebold (1980: 482–484), which include such improbable suggestions as the derivation of Gmc. **berō(n)* 'bear' from IE **ghwĕr-* 'wild animal' (Gk. *thḗr*, Lat. *ferus*, OCS *zvěrĭ*, etc.) or the assumption of a totally unattested base **ĝhwā-* (from **ĝhewə-* 'invoke, entreat' – listed by Pokorny [1959: 413] as **ĝhau̯e-* 'rufen, anrufen') to account for Gmc. *bō-ni-* 'request, prayer' (ON *bǭn*, OE *bēn*), hardly need to be discussed.

No irrefutable cogent argument can be derived from the evidence adduced to indicate that Gmc. *b-* reflects initial PIE **g^{w}h- (ĝhw-).*

(3) Initial PIE *$g^{w}h$- is delabialized before *u* and before resonants, as indicated by ON *guðr, gunnr*, OS *gūðea*, OE *gūþ* 'combat', OHG *gundfano* 'battle-flag', reflecting Gmc. **gunþ-* from PIE **$g^{w}hn̥t$-*.[6]

Unfortunately, the other evidence is rather weak, consisting mainly of Germano-Greek isoglosses, in which the Greek component is merely a lexical entry in Hesychius' glossary, e.g., MHG *gumpen* 'jump, hop, dance': Gk. *athemboũsa. akolastaínousa* (boisterous')[7] – or – Go. *gund* 'gangrene', OE *gund*, OHG *gunt* 'pus': Gk. *kanthúlē* 'tumor' (cf. fn. 3, *supra*). ON *grunr* 'suspicion': Gk. *phrḗn* 'midriff' (as seat of the emotions), *phronéō* 'think' is presumably to be rejected, as Gk. *phrḗn* seems to be linked with *phrázō* 'point out, explain' within Greek (Chantraine 1980: 1225, 1228).

(4) PIE *$g^{w}h$- has been assumed to be delabialized before PIE **o* in:
   (a) ON *geð* 'wits, disposition' < PIE *$g^{w}hodhyom$* (cf. OIr. *guidim* 'I pray', Gk. *póthos* 'longing');
   (b) ON *gandr* 'magic staff' < PIE *$g^{w}hondhos$* (from *$g^{w}hen$-* 'strike, slay');
   (c) MHG *gampen* 'hop', Norw. dial. *gamp* 'unwidely fellow' < *$g^{w}homb$-* (in ablaut with *$g^{w}hm̥b$-* in MHG *gumpen*).

   None of these examples are, however, convincing:
   (a) For ON *geð*, Seebold (1980: 459–460) made a fairly good case to establish that the basic meaning of the term was 'that which is fitting', which is precisely what George Lane had already suggested (1936: 23) to connect the term with OHG *gigat* 'fitting', OS *gigado* 'his like', from the root in OFris. *gadia* 'unite', MLG *gaden* 'fit, please, pair'. ON *geð* would then reflect Gmc. **ɣaðja-* < PIE **ghodhyo-* (cf. OCS *godŭ* 'right time', *godĕ byti* 'fit, please', *godĭnŭ* 'pleasing'; Pokorny 1959: 423–424), with a shift in meaning from 'what is appropriate for someone' to his 'characteristic disposition'.
   (b) As indicated in footnote 3 (*supra*), the original meaning of *gandr* focuses on the concept of magic and the etymology of the term is therefore to be sought along that line (de Vries 1957: 362).
   (c) The reservations made about MHG *gumpen*: Gk. (Hesychius gloss) *athemboũsa* 'exuberant' apply to MHG *gampen* as well.

As for the new example suggested by Markey (1980: 292), the name of hound of hell, ON *Garmr*, has been quite attractively explained in its mythological context by Bruce Lincoln (1979: 276–285) as a derivation from the onomatopoeic root **gher-* (Pokorny 1959: 439).

If, then, there is no cogent evidence of delabialization of PIE *$g^{w}h$-* before PIE **o*, should we assume that, in initial position, PIE *$g^{w}ho$-* developed into Gmc. **wa-* on the basis of Gmc. **warma-* 'warm': Lat. *formus*, Av. *garəma-* 'hot', Skt. *gharmá-* 'heat, warmth', etc.?

Several of the etymologies adduced to back up this assumption (e.g., Polomé 1948) should be abandoned, e.g., the derivation of Go. *wandus*, ON *vǫndr* 'rod, twig' from PIE *$g^{w}hen$-* 'strike';[8] the reconstruction of a PIE *$g^{w}hel$-* 'yellow' beside **ĝhel-* (Pokorny 1959: 429) to account for Du. *wouw* 'reseda luteola' and Lat. *fel* 'spleen' (cf. Seebold 1980: 468–469); the comparison of OHG *wahs* 'sharp' with Gk. *phoxos* 'pointed' (cf. Seebold 1980: 473–474). The case of Go. *wamba* 'belly': OInd. *gabháḥ* 'vulva' is more complex: the effort of Seebold (1980: 470–471) to assign the meaning 'groin' (G. 'Schenkelspreize') to *gabhá-* is rather unconvincing,[9] but even if the meaning 'vulva' is maintained for Skt. *gabháḥ*, it does not support linkage with Gmc. **wambō* 'belly, abdomen'. If the Old Indic

form has a labiovelar initial, it would rather be **g^{w}-* in PIE **g^{w}embh-/*g^{w}m̥bh-* 'plunge, sink down', also appearing in Gk. *báptō* 'dip', *baphḗ* 'dipping' – without nasal infix: ON *kaf* 'depth of the sea; diving', OSwed. *kvaf*; ON *kvefia* 'suffocate' (Mayrhofer 1956: 1.324; Pokorny 1959: 465–6; de Vries 1961: 296, 336; Chantraine 1968: 156, 164). That leaves the Germanic terms (Go. *wamba*, ON *vǫmb*, OE *womb*, OHG *wamba*) fairly isolated, because the alternate connection with Skt. *vapā́* 'omentum, skin investing the intestines', is improbable (Polomé 1948: 565; Skt. *vapā́* rather belongs to *vápati* 'shears, shaves' [terms for 'skin' are frequently deriving from roots meaning 'cut'; cf. de Witte 1950: 60ff.], as Mayrhofer 1976: 3.144 points out).

A direct connection between Skt. *gādháḥ* 'swallow', *gādhám* 'ford, shoal' and Gmc. **waða-* 'ford' (ON *vaða*, OE *wadan*, OHG *watan* 'wade') is fairly well excluded by the latter's correspondence with Lat. *vadum* 'ford': the Old Indic term seems to be an old 'rhyme-word' to PIE **wā́dhom* 'ford' (Mayrhofer 1956: 1.334).

This practically leaves Gmc. **warma-* as the only cogent example of the derivation of initial Gmc. **wa-* from PIE **g^{w}ho-*, but there is the alternate etymology deriving the term from the root **wer-* 'bwen' (Hitt. *u̯ar-* 'burn', OCS *varŭ* 'hear', etc.).

The parallel derivation of Gmc. **wō-* from PIE **g^{w}hō-* is also based on one isogloss: Go. *(dauns) wōþi* 'sweet (smell)', ON *þǿri* 'better', OE *wēðe* 'sweet, pleasant', OHG *wuodi* 'sweet, mild': Gk. *phṓtion. prosphilés, hēdú* (Hesychius)[10] – obviously rather weak evidence to reconstruct a PIE **g^{w}hōtyo-* (or **ǧhwōtyo-*) since there is no other trace within Greek of **phōt-* (~ **thēt-*) with the meaning 'sweet'. On the other hand, the alternate connection of the Germanic terms with OInd. (RV) *vāma-* 'dear, fair, noble' (de Vries 1961: 684) is unconvincing ('wertlose Wurzeletymologie', according to Mayrhofer 1976: 189).

Consequently, lack of sufficiently valid evidence compels us to conclude that the treatment of initial PIE **g^{w}h-* before mid-level back vowel (**ŏ*) in Germanic remains an unsolved problem: labialization to *b-* is improbable; delabialization to *g-* is unwarranted; assumed development to *w-* rests on disputable etymologies.

(5) The treatment of PIE **g^{w}h-* before front vowel in Germanic is even more difficult to assess, since there are *no* clear etymologies illustrating this development: from the parallelism of **g^{w}-* and **k^{w}-*, the maintenance of the labiovelar would be expected, but there is no evidence of initial Gmc. [ɣ^{w}] from **g^{w}h-* comparable to [k^{w}] in Go. *qiman* 'come' from PIE **g^{w}em-* or [x^{w}] in Go. *hveila* 'time' from PIE **k^{w}ey-* (cf. Lat. *quiēs* 'rest' [**k^{w}yē-*]: ON *hvīld* 'rest'). Labialization to *b-* is fairly well excluded; assumed development to *w-* rests on unacceptable etymologies such as:

(a) OHG *folle-wemōn. fluitare* ('swarm', Notker): Russ. *gómon* 'din, clamor', allegedly from PIE **g^{w}hem-* 'swarm' (Polomé 1948: 567). The Germanic terms, which also include OS *wemmanthi.* scaturiens ('gushing out [of blood], Prudentius glosses), MHG *wimmen* 'move, swarm', OHG *wimidōn, wimizzen* 'bubble up, swarm', Du. *wemelen* 'swarm', etc. (de Vries 1971: 827), belong presumably to the hardly etymologizable affective/onomatopoeic vocabulary, like *wabble, wobble*, and the like (Seebold 1980: 472).

(b) Go. *wairs* 'worse', ON *verr*, OE *wiers*, OHG *wirs*: OCS *gorijĭ* 'worse', with an assumed original meaning '(more) bitter', from PIE **g^{w}her-* 'burn' (Wood 1926: 103). Since OCS *gorijĭ* rather belongs with Gk. *kheírōn* 'worse' (< **gher-yes-*; cf. Sławski

1956: 321; Machek 1968: 176–177; Seebold 1980: 472), the Germanic terms have to be explained otherwise – perhaps, as Elof Hellquist (1948: 1398) suggested, from IE **wer-s-* 'high, upper' (cf. Skt. *várṣīyas-* 'higher', Lith. *viršùs* 'upper part, pinnacle'; Pokorny 1959: 1152) with the meaning 'over and above the right measure' (cf. OE *yfel* 'evil' [< **upé-lo-*] versus the superlative *ufemest, yfemest* 'highest, uppermost' [= Skt. *upamá-*]).[11]

(c) Go. *waila wisan* 'revel', OE *wesa* (Gl.) 'intemperate drinker', OHG *fírwesan* 'consume': Skt. *ghásati* 'consumes, devours, eats', allegedly from PIE **g^{w}hes-*. Though the separation of the Germanic terms leaves Skt. *ghásati* practically without etymology (cf. Mayrhofer 1956: 1.358; 1976: 3.699), they definitely belong with OInd. (RV VIII 4.8) *anú vāvase* 'has devoured', Avest. *vāstrəm* 'fodder', MIr. *fess, feiss* (**wes-tā*) 'meal', W *gwest* 'feast', ON *vist*, OHG *wist* 'food' under PIE **wes-* 'revel' (Feist 1939: 568; Pokorny 1959: 1171; de Vries 1961: 669). No valid answer can accordingly be given to the question of the development of PIE **g^{w}h-* initially before mid-level front vowel (**e*) in Germanic.

Notes

[1] Thus, the terms listed by Pokorny (1959: 203–204) under **der-* (2) und **ger-* (2) (1959: 383–385) are lumped together with those appearing under **g^{w}er(ə)-* (1959: 478). To be sure, as Pokorny (1959: 478) points out: "Gegenüber den ähnlichen Schallwurzeln *ger-, ĝăr-* ist nicht überall eine sichere Scheidung möglich", but a semantic difference is clearly recognizable ("für die ... unter *g^{u}er-* vereinigten Worte [ist] der Begriff der gehobenen Äußerung unverkennbar, bis auf die darum nicht sich anzugliedernde *dh-* Erweiterung" [e.g. Lit. *gerdas* 'shouting']). On the other hand, the reconstruction of a root **g^{w}hel-* "expressing various types of sound" (Speirs 1978: 46) ist strictly arbitrary: there is no direct evidence of the labiovelar, and it is assumed on the basis of a delabialized form **ghol-* reflected by Gmc. **galan-* (OHG *galan* 'sing, call, resound, cry', *nahtigala* 'nightingale', etc.) – with *-dh-* suffix in ON *galdr* 'song, charm spell, sorcery' – and of a labialized form **bhel-* (Lith. *bélsti* 'make a noise by banging or knocking', MHG *boldern* 'make a din', etc.) – with expanded forms such as **bhel-s-* in Skt. *bhāṣā* 'speech, language', Lith. *balsas* 'voice', etc. and **bhl-ew-* in Lith. *bliáuti* 'bleat, blubber, howl'. Again, a number of apparently unrelated roots (**ghel-* (Pokorny 1959: 428), **bhel-* (6) (Pokorny 1959: 123–124); **bhleu-* (Pokorny 1959: 158); etc.) are linked together with the flimsy excuse that "in words denoting sounds some have found a happy hunting ground for onomatopoeia". Wondering "how it is known what was onomatopoeic to the ears of Indo-European communities more than five thousand years ago". Speirs (1978: 46) wishes "to draw attention to the fact that the initial consonants, as reflexes of what we believe to have been labiovelars, follow the same pattern...". He could, of course, have done his home-work a bit better and also brought nicely together under labiovelar roots the set of forms meaning 'sticking out'; listed by Pokorny under **gher-* (3) (1959: 440) and **bher-em-* (1959: 142), e.g. G *Granne* 'awn' and *Brombeere* 'blackberry' (< IE **bhrē-mo-* 'thorny [shrub]'; cf. OE *brōm* 'broom, furze'); E *brim*, etc., or added to the alleged reflexes of PIE **k^{w}er-k^{w}-* (Speirs 1978: 52) OHG *felga*, OE *fielg* 'felly' < IE **pel-k̂-* (Pokorny 1959: 807). But pursuing Speirs' reasoning ad absurdum, there would be few roots left with original labials, dentals or velars, while labiovelars would astonishingly proliferate, and one wonders with his promised revision of the current ablaut theory (Speirs 1978: 69) what will be left of the generally accepted Proto-Indo-European phonological system?

[2] Cf. Moulton (1972: 171): The voiced labiovelar /g^{w} ~ɣ^{w}/ "seems to have been a unit phoneme in Pre-Gmc, but not in PGmc". The voiceless labiovelars /k^{w}/ and /x^{w} ~h^{w}/ "may well have been the consonant clusters /kw/ and /xw ~hw/ rather than unit phonemes". Actually, the consonant shift yielded [x^{w}] from IE **k^{w}*, except under the conditions of Verner's Law when [ɣ^{w}] resulted, but [x^{w}] became gradually [h^{w}] in various environments in the different Germanic dialects (where their distribution parallels that of /x~h/ from IE **k*; Moulton, 1972: 173).

[3] It is not clear whether Markey (1980: 292) assumes any direct relationship between these two terms, but obviously semantic considerations would rather exclude it. Wood (1926: 106–107) connected Da. dial. *vann*, OE *wenn* 'wen', MDu. *wan* 'tumor', with OHG *gunt* 'pus', Go. *gundr* 'gangrene', and derived them all from IE **gʷhen-* 'swell'. As Seebold (1980: 462) points out, the basic meaning of the root **gʷhen-* with which Skt. *ghaná-* 'hard, firm, thick', Lith. *ganà* 'enough', Gk. *euthēnós* 'thriving, flourishing' are usually connected, is rather 'coagulate, curdle' (hence 'thick, compact', and 'thrive', respectively), which does not strengthen his semantic argument against the derivation of the Germanic term for 'pus' from this root. The alternate link with the Hesychius-gloss *kanthúlē* 'swelling tumor' remains disputable, and other explanations are plausible for the Greek term (Chantraine 1970: 492). As for NWGmc. **wanja-* > OE *wenn* 'wen', MLG *wene* 'tumor, wart', etc., it has also been connected with OHG *wanast, wenist* 'rumen', which is not more convincing! ON *gandr*, on the other hand, should be glossed 'monster, magic', according to Seebold (1980: 458), who rejects the meaning 'wand', referring in a footnote to Elof Hellquist (1893), which he could not consult. This information, like that of most etymological dictionaries, is presumably based on Sveinbjörn Egilsson, who states (1931: 170) that 'wand' is the basic meaning of the term, referring specifically to Hellquist (1893) and identifying *gandr* with *vǫlr*, a round thick staff described as 'redskab til udøvelse af völvers virksomhed'. This magic staff is used by fortunetellers and then called *spágandr* (cf. Vǫluspá, st. 29: *fekk spjǫll . . . ok spá ganda*); the phrase *vitta ganda* means 'cast a spell', and in Vǫluspá, st. 22, it applies to Heiðr plying witchcraft. Confirmation of the meaning 'staff' is sought in the compound *gandreið* 'ridt på en stok', but Jónsson (1931: 170) has to acknowledge that in the kenning *skjalda gandreið gandr* has to be interpreted as a 'harmful being', i.e. the shield's wolf. This ties in with the fact that the wolf is the witches' mount and has led Hugo Gering (1927: 28–29) to the conclusion that 'wolf' must be the older meaning, from an original etymon designating a magic being ('Zauberwesen') and applying secondarily to objects such as a magic wand. Obviously such an assumption makes derivation from IE **gʷhen-* 'hit, strike, kill' less plausible. It is, however, preferable to consider 'magic object' as the original concept, in which case the connection with ON *ginn-* 'outstanding', e.g. in *ginnregin* 'supreme gods', *ginna* 'fool, delude, practice magic', *Ginnarr* (= *Óðinn*), lit. 'magician', Run. *ginArunAR* (7th c.) 'magical powers' (de Vries 1930) would deserve further consideration (cf. de Vries 1961: 155, 167–168).

[4] Though Markey (1980: 286) does not refer to Steensland (1973), his remarks about the thesis postulating primordial palatals and labiovelars at the exclusion of a separate velar series are quite relevant: "Structurally speaking alone, the velars are a necessary component in the opposition between palatals on the one hand and labiovelars on the other: their elimination prohibits realization of the palatal quality of the palatals and the labial quality of the labiovelars".

For a review of Steensland's work, cf. Adolf Erhart (1974: 68–69).

[5] OBret., Corn. *louuern* 'fox' reflects **lowerno-* and is not related (Campanile 1974: 74).

[6] For a thorough analysis of the phonological and morphological problems involved, cf. Seebold (1980: 451–457).

[7] Neither Frisk (1960), nor Chantraine (1968) lists Gk. (Hesych.) *athemboũsa*, which does not appear in Liddell-Scott (1940–1968) either. As is well-known, in Hesychius many glosses without ethnic reference or specific localization are not actually Greek.

[8] Rather than following Seebold (1980: 466) in his acceptance of Jost Trier's connection with OInd. *vánas-*, interpreted as 'cluster of foliage' (G. 'Laubbüschel') in RV X 172.1, and further with Skt. *vána-* 'forest, tree', we would give preference to the traditional derivation from IE **wendh-* 'turn, wind, twine' (Pokorny 1959: 1148), as the term corresponds to OHG *want* 'wall' – made of intertwined twigs plastered with loam (cf. ON *vandahūs*, lit. 'wickerwork-house'); this etymology is further confirmed by such terms as Swed. dial. *vann* 'tendril' (of pea plant).

[9] In the passage quoted from the Vājasaneyi Saṁhitā XIII. 24, there is no cogent reason to reject the meaning 'penis' given to *muṣṭi-* (Mahīdhara; cf. Monier-Williams 1899: 824b): the term is related to *muṣkaḥ* (RV) 'testicle', *muṣkáu* (AV) 'pudenda muliebra' (Mayrhofer 1963: 2.657–658) and belongs to the common semantic transfer of 'mouse' to parts of the body (e.g. Lat. *musculus* 'muscle'; cf. De Witte 1950: 67, 350); the sexual connotations of those derivations have been demonstrated by Hermann Güntert (1913), and parallels like MHG *miuselîn* 'membrum virile' ~ 'little mouse', as well as the symbolism of obscene gestures with the clenched fist (e.g., in the Mediterranean area) illustrate the fact that the coexistence of meanings like 'penis' and 'fist' on the basis of their **Mäuse-*

schaft ('mousedom'; Thieme 1951: 214, fn. 1) is not excluded. Skt. *gabhe muṣṭim ataṃsayat* accordingly refers to the act of copulation and *gabhá-* is the 'vulva', literally: the 'hollow', the 'recess', since *gabhá-* cannot be separated from *gámbha-* 'depth', *gambhirá-* 'deep', Avest. *ǰafra-* 'deep', *ǰafnuš* 'hollow, recess'. That this is a common way to designate the female genitalia is abundantly illustrated by the relevant terminology (De Witte 1950: 272ff.).

[10] The doubts expressed by the 1862 editor of Hesychius' lexicon, quoted by Seebold (1980: 472, fn. 92) are apparently not shared by Liddell and Scott (1940: 1969), but neither Frisk (1970), nor Chantraine (1980) lists *phôtion*.

[11] Cf., e.g. De Vries (1961: 657).
The derivation from **aṷer-* 'lower', proposed by Seebold (1980: 472), is unconvincing: Skt. *avarah* 'inferior' is the comparative of *áva* 'down' versus the superlative (RV) *avamá-* 'undermost, lowest', and RV *avár* 'down' is a locative adverb in *-r*; there is accordingly no ground to reconstruct a Proto-Indo-European theme **H$_2$w-er-*, enlarged by *-s-* in Germanic, on the basis of such evidence.

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All our 'yesterdays'

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In a short monograph of his later years Karl Brugmann (1917) pointed out that Indo-European possesses a common etymon for 'yesterday' but not for 'today' or 'tomorrow'; for the latter, parallel transparent means of expression predominate, as is still the case with the English terms. Thus 'today' is typically made up of some demonstrative pronominal element and a word for 'day', as in Skt. *a-dyá*, Hitt. *anisiwat(ti)* or *kedani siwatti*, Arm. *ays-awr*, Gk. σήμερον (< *κι̯-āμερον)[1], Alban. *sot* (< *k̂yā-dīti*), Lith. *šiañ-dien*[2], OCS *dĭnĭ-sĭ*, Russian *segó-dnja*, Irish *in-diu*, Welsh *he-ddyw*, Lat. *hodiē* (Faliscan *foied*), Gothic *himma daga*, OSax. *hiu-diga*, OHG *hiutu*[3], ON *ī-dag*. Equally consistently, 'tomorrow' is derived from or cognate with 'morning', e.g. Skt. *śvás* (cf. Avest. *sūrəm* 'early in the morning'), Hitt. *lukkatti (siwatti)* or *lukkatta (siwatti)* (*lukat-* 'morning'), Arm. *vałiv* (*vał* 'early'), Gk. αὔριον (**awsr-* 'dawn', as in Lith. *aušrà*, cognate with ἠώς[4]), Lith. *rytój* (*rýtas* 'morning'), Russian *zá-vtra* (*útro* 'morning'; cf. OCS *za ustra*, cognate with Gk. αὔριον), Irish *imbārach*, Welsh *a-vory* (Welsh *bore* 'morning'), Lat. *crās* (whether cognate with Avest. *sūrəm* 'early' or with Ved. *śárvarī* 'dusk', Hitt. *karū* 'early', *kariwariwar* 'at daybreak'[5]), Goth. *du maúrgina* (whether root-related with Lith. *márgas* 'bright, variegated' or OCS *mrakŭ* 'dimness, darkness').

Occasionally a pendant relationship 'evening' : 'yesterday' is found, e.g. Lith. *vākaras* : *vākar*, or Russian *véčer* : *včerá*, or German dialectal *jennabend* or archaic *nächt* 'yesterday'; cf. 'eve of' = 'day before'. But in the main, except in Baltic and Slavic, the overpowering association of 'tomorrow' with 'morning' has been unable to dislodge, let alone supplant the ancient terms for 'yesterday', not only in archaic but also in heavily innovated forms of Indo-European.[6]

Klaus Strunk (*IF* 73. 1968: 309–310) has suggested that the archaism of words for 'yesterday' ties in with the ancient abilities of the Indo-European verb to denote past time, and that conversely the later nature of terms for 'tomorrow' is related to the gradual devising of future tense expression in the individual languages. If Proto-Indo-European was incapable of expressing 'tomorrow' lexically as well as grammatically, such a semantic lacuna would have robbed any word for 'yesterday' of some of its antonymity, since its only contrast term would be 'today'; it could have expressed the past only as related to the present, not as also opposed to the future, in a binary rather than ternary semantic structure. It is, however, improbable that verbal grammar is to blame for this state of affairs. More probably we are dealing with a shift in conceptual emphasis, from objective side-view of event-sequences to the subject's own time-immersion, from a 'third-person' to a 'first-person' approach. Clearly there must always have been objective means of expressing '(on) the next

day', such as Hitt. *parā* UD. KAM-*ti* or Skt. *(a)paredyús*; here days are simply lined up in calendaric fashion, as when Wulfila translates *Luke* 7.11 ἐγένετο ἐν τῇ ἑξῆς 'it came to pass the day after' (ἑξῆς literally 'in sequence') by *warþ in þamma afardaga*. But in *Corinthians* 1.15.32 (φάγωμεν καὶ πίωμεν, αὔριον γὰρ ἀποθνήσκομεν) the first-person speaker is very subjectively involved, and 'the next day' turns into 'tomorrow': *matjam jah drigkam, unte du maurgina gaswiltam* 'let us eat and drink, for tomorrow we die!'. Anticipation of the morning after identified the morrow with the coming day and launched the innovational term.

By contrast, then, the old term for 'yesterday' should have been 'dispassionate', with no hint of a 'dreadful past' or emotional involvement with 'all our yesterdays'; it must have meant simply 'the other day'. Its well-known inventory of forms comprises Skt. *hyás*, Pahlavi *dīk*, Persian *dī(g)*[7], Gk.*(ἐ)χθές*, *χθιζά*, Elean *σερός*, Alban. *dje*, OIr. *in-dé*, Welsh *(yd)doe*, Lat. *heri* (*hesternus*), Goth. *gistradagis*, OE *geostra, giosterdæg*, OHG *gestarōn*, ON *(ī) gær* or *(ī) gjār*. Brugmann (1904: 72) postulated for the first part the demonstrative pronominal stem **gho-* or **ghi-*, and Vittore Pisani (1925: 637–643) in a youthful opus supplied a full reconstruction **gh(e)-diḗs*, variations on which have subsequently entered the handbooks.[8] As an alternative to such glimpsing of the etymon of Lat. *diēs*, already Brugmann (1904: 72, fn. 10) suggested that a suffix *-yo-* might be involved, denoting appurtenance or adjacency to '(to-)day'; Franz Specht (1944: 201–205) later came up with a formula **ĝh-yes* containing the equivalent of the primary neuter comparative suffix, and saw its hypertrophied expression in **ĝh(y)es-t(e)r-* (Goth. *gistra-*, Lat. *hesternus*) with the secondary comparative suffix. Specht was also aware that these suffixes can have a binary contrastive meaning, as in Ved. *návyas-* 'new' vs. *sányas-* 'old', or in Lat. *noster* vs. *vester*. For **ĝh-* he stuck with the pronominal explanation, trying weakly to explain Gk. *χθές* and the Celtic forms via some sort of deictically marked allomorph **ĝh-t-*; the meaning would thus have been something like 'this (day) as opposed to today', i.e. 'the other day'.

Jan Otrębski (*KZ* 84. 1970: 85–87) inverted the semantic problem completely by insisting that, in view of OCS *vĭčera* 'yesterday' beside *večerŭ* 'evening', the base meaning had to do with either 'shine' or 'get dark', hence connecting either Goth. *skeinan* 'shine', Gk. *σκιᾱ́* 'shadow' or a root **(s)ĝhei-(s)-* metathesized ad hoc into **ĝhesi* (Lat. *heri*).

No further progress was possible without squarely confronting the matter of the initial consonantism, which is part and parcel of the 'thorn' problem. Here the many 'modern' treatments starting with Paul Kretschmer's article (1931/32) have been of uneven value. Some theoreticians, enamored of their own phonological circularities, have either spawned monstrosities[9] or reverted to ground zero.[10] Others have chosen to stay mostly reportorial (J. Gunnarsson, *NTS* 43. 1971: 52–55). Even those who record the most plausible reconstruct, **(dh)ĝh(y)es*, either leave it unanalyzed (Merlingen 1957: 60) or pronounce it unanalyzable (J. Schindler, *Die Sprache* 23. 1977: 32). With this formula the inventory can be presented as follows:

**dhĝhyes* > OIr. *(in-)dé*.
**dhĝhiyes* > Welsh *(yd)doe*.
**dhĝh(y)es* > Gk. *χθές*[11] (cf. e.g. **dhĝhōm* > *χθών*[12]).
**(dh)ĝhyes* > Elean *σερ(ός)*, Skt. *hyás*.
**(dh)ĝhyes-tro-* > Goth. *gistra-*.[13]
**(dh)ĝhyēs* > ON *(ī)gær*.

**(dh)ĝhiyēs* > ON *(ī) gjār* (Gunnarsson, *NTS* 43. 1971: 54–55).
**(dh)ĝh(y)es* > Alban. *dje.*
**(dh)ĝh(y)esi* > Lat. *heri.*

The pattern is consistently that of the locative case of an *s*-stem noun **dhĝhyes-*. This noun may contain the primary 'comparative' suffix *-yes-* (as suggested by Specht) appended to a root **dhĝh-* in the weak grade. The word basic to the derivation can have belonged to any stem class (cf. Gk. ταχ-ύς, **θαχ-ι̯οσ- > θασσο(σ)-*, or **κῡδαρ, κῡδιο(σ)-*). The noun in question is in all probability the etymon of English *day*,[14] reconstructible variously as **dhoĝho-* (Goth. *dags*, ON *dagr*, OHG *tac*), **dhōĝhes-* (OE *dæg*), **dhōĝhr-* (OE *dōgor* 'day', ON *dǫgr* 'day or night'), **dhōĝhn-* (ON *dǫgn* 'day and night, 24-hour period').[15] The commonly assumed cognate is Ved. *áhar-, áhan-, áhas-*, Avest. (gen. pl.) *asnąm* 'day', where an originally marginally accented paradigm *(*dhóĝhr̥) : (dh)ĝhn-és* (cf. *ásṛk : asnás* 'blood') may have been rehabilitated in Indo-Iranian as a columnal *áhar : áhnas*, perhaps abetted by the synonymous rhyme-term **ayar-, *ayan-* (Avest. *ayarə*, gen. *ayąn*); it can likewise involve a 'movable dental' in the manner of Ved. *áśru* vs. Gk. δάκρυ 'tear', or Lith. *ìlgas* vs. Ved. *dīrghá-* 'long', or Ved. *amā́* 'at home' vs. *dám-* 'house'.

An opposition **dhóĝhos* or **dhóĝhr̥ : *(dh)ĝh(i)yós* (with **[dh]ĝh[i]yés* as suffixless locative) is quite parallel to Gk. κῦδος or **κῦδαρ : *κῡδιος*. The 'comparative' suffix involves in origin not a balanced binary pair of opposites but signals the marked member of the opposition, even as in Greek εἷς vs. ἄτερος (Ionic ἕτερος) reflect IE **sems* vs. **sm̥tero-* (cf. Homeric χωλὸς δ' ἕτερον πόδα 'lame in one foot' [vs. the other]). In the same way **(dh)ĝh(i)yos* denoted the one of two adjacent days which contrasted with 'now', thus 'the other day' (cf. Gk. θατέρᾳ 'on the morrow'). The meaning 'yesterday' (rather than 'the next day') is presumably a conventional one, due in large measure to the pre-emption of futurity by the 'morrow' words. The traces of an alternative meaning 'the next day' in Germanic need not be desperately explained away (thus e.g. Brugmann 1917: 10–15; Strunk, *IF* 73. 1968: 309–310); thus Wulfila's (*Matthew* 6.30) *himma daga . . . gistradagis* translating σήμερον ... αὔριον speaks of hay that is on the stalk one day and ends up in the oven the next day, and the Eddic (*Hamdismál* 30) *nū eða ī gær* wonders whether death will come 'now or the next day'; *himma daga . . . gistradagis* opposes **dhoĝho-* to a doubly characterized figura etymologica **(dh)ĝhyestro-dhoĝho-*, reminiscent of the manner in which French *aujourd'hui* twice contains derivatives of the etymon of Lat. *diēs*. The literal meaning is, however, 'on the day which is the other day in relation to now'.

Notes

1 Resembling σῆτες, Myc. *ssa-we-te* (< **κι̯ᾱ-Fετες*) '(in) this year'.

2 Besides Alban. *si-vjet* (< **ḱyéy-wetesi*), Lith. *šĩ-met* '(in) this year' (cf. M. Huld, *KZ* 98. 1985: 102–103). Cf. e.g. Finnish *tänä vuonna* (< **vuodna*), Estonian *tänavu(de)* (< *tänä voodena*) '(in) this year', besides Finnish *tänä päivänä*, South Estonian *täämbä* 'to-day' (all in the essive case).

3 Parallel to Lat. *hodiē* and OHG *hiutu*, German *heute* stand Lat. **hōri* or **hōrō* (< **hō-iōr-*) and OHG *hiuru*, German *heuer* (< **hiu jāru*) '(in) this year' (cf. Lat. *hōrnus, hōrnōtinus* 'this year's', like German *heurig* from *heuer*); the ongoing innovational process is seen in Lat. *hōc annō* > Spanish *ogaño* 'this year'.

4 Perhaps also with ἦρι-, ἠέρι· 'early., ἀήρ 'dimness, mist', αὔρη 'morning breeze' (cf. P. Kiparsky, *Language* 43. 1967: 624–626).

[5] The Hittite words, however, are better connected with cognates of English 'grey' such as ON *grȳiandi* 'dawn'; cf. Milton's 'while the still morn went out with sandals gray'. From Lat. *māne* '(early in the) morning' (cf. *crās māne* 'early tomorrow morning') are derived the Romance words for 'tomorrow'.

[6] Cf. Lat. *heri* > French *hier* (Italian *ieri*, Spanish *ayer*), vs. *aujourd'hui* or *demain (*dē māne)*.

[7] 'Avest. *zyō*' of many sources is a lively ghostword.

[8] E.g. **ĝh(i)d(j)es* and **ghi-diwes* in E. Schwyzer (1939: 631 and 326 respectively).

[9] **g$^{z}_{i}$hyés* in E. Benveniste, *BSL* 38. 1937: 144.

[10] **ĝhi̯es* in J. Kuryłowicz, *BSL* 68. 1973: 102.

[11] The detail of Gk. χθιζά remains unclear; both Skt. *a-dyá* 'today' and πρωϊζά = πρώην 'the day before yesterday' have afforded material for comparison, on different chronological levels. Most likely, however, is **dhĝhis-* as zero grade (saṁprasāraṇa) suffix variant of **dhĝh(y)es-*, with suffix as in κρύβδα 'secretly' (cf. Specht 1944: 205). χθιζός is a secondary adjectivization of χθιζά (cf. χθεσινός).

[12] Metatheses of dental and guttural, postulated in such clusters for Greek and Celtic, seem to have operated sporadically also in non-contact environments: Gk. δάκτυλος < *δατκύλος < *τκαδυλός (cf. J. Puhvel, *IF* 81. 1976: 26; Puhvel 1981: 350), or Toch. B *kantwo* 'tongue' < **dn̥ghw-*. Cf. the similar instances of labial and guttural (Gk. σκεπ- < **speḱ-*; Lith. *kùmstė* 'fist' < **pn̥k^{w}st-*).

[13] Matched by Lat. *hesternus* < **(dh)ĝh(y)es-tro-no-* (cf. Skt. *hyástana-* and Gk. χθεσινός).

[14] The possibility of such a tie-in was first hesitantly suggested by W. Brandenstein (*Glotta* 25. 1936: 29) and worked out in more detail in a study by M. Durante (1950: 248); Durante (1950: 247) brilliantly connected Gk. φθέγγομαι 'produce voice' with Lith. *žvéngiu* 'neigh' as reflecting *(dh)-ĝhweng-*.

[15] Cf. also the vṛddhi-like derivational long grade in Goth. *fidur-dōgs* 'four days old', comparable to Skt. *śatá-śāradas* 'of a hundred autumns'.

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Verbi forti e verbi deboli in Germanico

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1. E' noto che l'antico sistema dei verbi forti (Vf) germanici (germ.) ha subìto una notevole riduzione in tutte le lingue della famiglia – talvolta in modo addirittura drammatico, come nel caso dell'afrikaans, lingua creolizzante nella quale una coniugazione perifrastica ha sostituito pressocché totalmente le forme preteritali tanto della coniugazione forte quanto della debole. Per lo scopo di questo scritto possiamo tuttavia lasciare da parte il più vasto problema delle forme perifrastiche che sostituiscono quelle sintetiche poiché esso concerne tanto i Vf quanto i verbi deboli (Vd) e non costituisce quindi una discriminante tra i due tipi verbali.

Forme deboli hanno sostituito per estensione analogica molti antichi preteriti (Pret.) e participii passati (PPa.): ags. *slēp* → m.ingl. *slēp/slēped(e), slepte* → ingl. *slept*, m.neerl. *gewesen* → neerl. *geweest*, ted. *wob* → *webte, boll* → *bellte, buk* → *backte*, per citare gli esempi di Jacob Grimm che lamentava la perdita delle forme forti, le quali "schöner und deutscher singen" (cfr. Augst 1975: 274).

I fenomeni implicati in questo combiamento sono stati ampiamente studiati in dimensione diacronica dalla linguistica storica e in sincronia soprattutto dalla psicolinguistica (mancano, invece, osservazioni adeguate dal punto di vista sociolinguistico: vd. fenomeni di creolizzazione di lingue germaniche).[1]

Si è constatato che bambini parlanti inglese passano attraverso quattro fasi nell'apprendimento delle forme preteritali:

(a) produzione di pret. irregolari (irreg.) dall'alta frequenza (*go, drink, eat, give, can, may, have, be*, ecc.: in una registrazione di 49 ore di interazione linguistica con bambini tra i 16 e i 24 mesi Slobin ha registrato 292 forme di pret.irreg. contro 99 regolari (reg.): vd. Slobin 1971: 220). Queste forme vengono apprese come lessemi (*items*) isolati: "each irregular verb [. . .] is a case unto itself; no rule covers more than few words and in many cases no more than one word" (McNeill 1970: 85). Grazie alla loro altra frequenza d'uso queste forme sono prodotte addirittura prima di quelle regolari (vd. Brown 1973: 220);

(b) acquisizione della regola per la formazione del pret. reg.: *love* → *love-d, work* → *work-ed*;

(c) (iper)generalizzazione della regola (b) con eventuale produzione di forme ipercorrette: *I bringed, I goed* e anche *I done* per *I brought, went, did.* I cambiamenti analogici mirano ad introdurre forme più regolari, cioè non marcate, prevedibili, o, per impiegare i termini di P. Kiparsky, esse mirano ad una semplificazione delle regole (Kiparsky 1971: 174) con giustappunto l'esempio di *brought* → *bringed*).

Si osserverà che anche i lapsus linguistici mostrano la stessa tendenza ad eliminare apofonie (umlaut e ablaut): Esper 1973: 58;

(d) acquisizione di ulteriori forme di preteriti irregolari in una fase successiva dell'apprendimento linguistico.

Lo stesso vale anche per il tedesco, dove esiste per il parlante e sopratutto per il bambino che apprende la lingua una dicotomia fondamentale tra forme regolari e irregolari; queste ultime, esattamente come in inglese, comprendono Vf, AUX, modali: tutti i Verbi non-deboli: cfr. Augst (1975: 262). E, come in inglese, gran parte dei verbi irregolari appartiene al 'basic vocabulary' di alta frequenza d'uso.

Se questi dunque sono i fatti (qui riassunti sommariamente), è importante per le osservazioni che seguono che non tutte le forme irregolari mostrano lo stesso grado di irregolarità. Slobin ha osservato che un paradigma quale *bring/brought* è ripetuto dal bambino in fase di apprendimento più regolarmente delle forme preteritali basate esclusivamente sull'alternanza vocalica (*give/gave*): "it is as if the final dental consonant is enough of an anchor to the regular rule that the child can tolerate a vowel in past tense inflection" (Slobin 1971: 220).

Lo stesso si può dire per le forme corrette di preteriti in *-d* con ablaut: *told, sold*; infine forme come *kept* risultano ancora più facili in quanto si avvicinano molto a quanto ci si aspetterebbe secondo gli schemi regolari (**keeped*). Si noti che in questo ultimo caso la forma del preteriti coincide con quella del PPa.: la polimorficità del paradigma basato su tre forme distinte (originariamente quattro) viene così in ogni caso a ridursi – una tendenza ben nota nella grammatica inglese: cfr. *to cut, to let* (una sola forma), *to get, to sit, to find* ecc. (due forme). Qualsiasi siano state le evoluzioni fonetiche alle base di tale tendenza, essa costituisce il motivo della sostituzione di *spoke, broke* a *spake, brake* regolarmente alternanti (ags. *spræc, bræc*) sulla base del PPa. *spoken, broken*: la sostituzione di *I done* a *I did* (osservata in bambini americani) è dunque lo stesso tipo di regolarizzazione, meno radicale di **I doed* (cfr. le forme effettivamente registrate *goed, comed, bringed*, ecc.).

Forme ipercorrette (e ipercaratterizzate) come quelle riportate da Lejosne (1982: 164) *sprongde* "he sprang", *kwamste* "he came" per il dialetto di Westhoek (distretto di Dunkerque) mostrano in un linguaggio di adulti la stessa tendenza a (iper)generalizzare un paradigma regolare che abbiamo visto prima alla fase (c) dell'apprendimento linguistico.

Questa tendenza è ovviamente alla base anche dell'estensione del paradigma debole ai danni del forte osservata in diacronia.[2] A causa degli accidentali sviluppi fonetici del tipo ora ricordato alla nota 2, i paradigmi forti divengono sovente del tutto imprevedibili: la tendenza a sostituirli con forme deboli è da ricondursi alla tendenza universalmente valida in morfologia "marcato → non marcato" (cfr. Mayerthaler 1980). La tendenza ad abbandonare il paradigma forte e la sua incapacità (relativa: vd. più oltre) a produrre nuove forme ebbe come risultato di sostituire all'antica opposizione fra due paradigmi (forte e debole) egualmente vitali e produttivi una nuova opposizione tra regolare e irregolare: questa seconda serie comprende – come osservavo più sopra – tutti i verbi non-deboli (:". . . eine Klasse der starken Verben spätestens seit der Aufklärung gar nicht mehr gibt. [. . .] Eine Gruppe 'starke Verben' gibt es in seiner [= des Sprechers] Kompetenz nicht, sondern nur die Unterscheidung regelmäßig (d.h. sprachhistorisch schwach) und unregelmäßig. Unter die letzteren fallen in historischer Hinsicht die starken Verben, die Präterito-präsentia, die Wurzel- und Suppletivverben": Augst 1975: 263).

E' tuttavia da notare che *sprongde* e *kwamste* conservano ancora la vocale apofonica originaria: una soluzione meno radicale delle forme completamente regolarizzate *comed, bringed. Sprongde* e *kwamste* si collocano allo stesso livello di *told, sold*: ablaut + desin. debole.

Un problema in parte diverso è rappresentato dalle coppie tipo neerl. *zwoer/zweerde* 'I/he sweared', *dolf/delfde* 'I/he delved' che hanno forme forti e deboli (ma PPa. forte: *gezworen, gedolven*). Dal punto di vista diacronico esse rappresentano evidentemente l'estensione del paradigma debole, ma la persistenza di preteriti forti mostra ancora il conflitto tra la fase antica e quella più recente: le forme deboli non si sviluppano da quelle forti per evoluzione fonetica (salvo forse alcuni casi di Vf in dentale tipo neerl. *heten*: pret. forte **hët* deb. *heette, scheiden*: pret. forte **scheed* deb. *scheidde*): non c'è passaggio fonetico tra *zwoer* e *zweerde* e la creazione di *zweerde* avviene 'ex abrupto' per analogia. Le relazioni fra i due tipi dovrebbero esser esaminate per ciascun singolo periodo storico in termini di rispettive frequenze. Le forme forti di più alto rango di frequenza sono state ovviamente le più resistenti ai processi di neoformazione analogica.

2. Molto meno noto è al contrario il fenomeno inverso, cioè l'estensione di forme forti di pret. e PPa. a verbi originariamente deboli: vd.p.es. fris. *erfde → urf* (ted. *erbte*), *merk(e)de → murk* (ted. *merkte*), neerl. *beliëde → beleed, wijzde → wees* e *sande → zond, schande → schond, fluit(e)te → floot, blink(e)de → blonk, clink(e)de → klonk*, ecc. (infin. *beliën > belijden, wijzen, zenden, schenden, fluiten, blinken, klinken*). A prescindere da quali siano stati i singoli sviluppi fonetici,[3] questi esempi mostrano che il passaggio si è verificato quando la forma fonica della radice verbale mostra una forte analogia con serie tipiche dei Vf: cfr. p.es.fris. *kerven* 'tagliare', ora con pret. forte *e* deb. *kurf/kerfde*!, neerl. *kijken* 'guardare', *krijgen* 'ottenere' ecc. (secondo la I classe forte), *vinden* 'trovare', *zingen* 'cantare' (secondo la III classe), ecc. Come osserva giustamente Mossé per l'inglese, ". . . des timbres [. . .] étaient sentis comme appartenant spécialement aux verbes forts. Ainsi quand par hasard un verbe faible avait un de ces timbres, avait-on tendance à en faire un verb fort" (Mossé 1960: 105). Cfr. in ingl. contemporaneo *I wore, tore* sul modello dei Vf. *I swore, I bore* (infin. *to swear, bear*)!

In effetti, esperimenti condotti su anglofoni americani adulti hanno mostrato che esiste ancora una certa competenza linguistica capace di generare nuove forme forti di verbi inventati come *to gling* o *to bing* (il che corrisponde alla fase *d*) dell'apprendimento, vista più sopra), mentre bambini all'incirca di 6 anni rivelano maggiori difficoltà nel creare forme 'regolarmente' apofoniche (fase *c*): cfr. Berko (1958: 150–177). Analogamente un esperimento chiedeva a studenti tedeschi di restituire l'apofonia di antichi Vf divenuti deboli nel medio alto ted., quali *schmiegen* e *schweifen*: accanto a un 31% di risposte corrette (*schmog geschmogen*, come *bog gebogen* da *biegen* o *bot, geboten* da *bieten*), 30% delle risposte era costituito da forme analogiche (*schwiff geschwiffen*, come *griff gegriffen* da *greifen, biß gebissen* da *beißen*, in luogo di *schwief geschweifen*) e 39% da risposte erronee principalmente per quei verbi che risultano difficilmente associabili a una qualche serie foneticamente simile ('rhyming series'): caso di *schroten* 'macinare', la cui sola possibile associazione può esser data da *stossen*; vd. Augst 1975: 269–273.

Risulta da questi esperimenti che esiste dunque ancora una sorta di competenza linguistica dei Vf come sistema grammaticale, basata sull'analogia rimante con verbi più usuali (vd. i 'rapports associatifs' di Saussure).

Dal punto di vista diacronico è facile concluderne che la creazione di nuovi preteriti e PPa. forti risale ad un periodo in cui il sistema dei Vf era ancora capace di dar origine a nuove formazioni, di cui l'esempio più evidente risulta ancora l'antico prestito germ. **screiƀan* 'scrivere', flesso come appartenente alla I classe forte. Si tratta della stessa tendenza verso neoformazioni analogiche che abbiamo visto a proposito dei pret. deboli di Vf: i parlanti tendono a serie regolari di forme già presenti nella loro competenza (e dotate normalmente di alta frequenza). Dal punto di vista qualitativo non c'è differenza tra i due tipi di fenomeni, cioè tra Vf che passano a deboli e Vd che passano a forti.

La creazione delle nuove forme di pret. forte come *urf* et sim. può esser stata influenzata dall'esistenza in quasi tutte le lingue germaniche – almeno a un certo punto dello sviluppo storico – di coppie (per i Vf) del tipo fris. *griep/grypte* (*gripe* 'greifen'), *swol/swolde* (*swolle* 'to swell, schwellen' e il già citato *kurf/kerfde* (*kerve* 'to carve'), neerl. *stiet/stootte* (*stoten* 'stossen'), *zwoer/zweerde* (*zweren* 'schwören, to swear', e anche *korf/kerfde* (*kerven* 'to carve'), *dolf/delfde* (*delven* 'to delve'), *krees/krijste* (*krijsen* 'gridare'), ecc. Queste coppie possono aver influenzato la creazione di coppie analoghe anche per i Vd dalla struttura fonologica rimante con modelli forti: cfr. ted. *pries, glich* (da *preisen, gleichen* sul modello di *bleichen* -- che ha *blich/bleichte*! –, *reissen, schmeissen* ecc.), *frug/fragte, lud* (*/ladete*) sul modello di *fahren, graben, backen* – che ha *buk/backte*! – ecc.

Questa serie è evidentemente meno ricca di quella opposta (cioè Vf → Vd); comunque essa esiste in diverse tradizioni linguistiche germaniche (neerl., fris., ted., nynorsk[4]) e occorre notare che le forme forti analogiche di solito non sono attestate per i periodi più antichi: mancano esempi in gotico, antico sass., antico fris., antico alto tedesco; l'ags. conosce solo il caso di *rīnan* (denominativo da *regn* 'pioggia'). Ciò testimonia di una lunga vitalità del modello forte, o addirittura di una sua ripresa: nel paragrafo seguente cercherò di fornirne i possibili motivi. Per il momento sarà da concludere che dal punto di vista sincronico e tipologico coppie del tipo *murk/merk(e)de* debbono esser considerate allo stesso livello di *griep/grypte* (vd. schema).

Si prenda anche una forma come Westhoek *donste* (accanto a *danste*, dal Vd *dansen* 'to dance': Lejosne 1982: 163): la si può spiegare solo riferendosi a un momento in cui la III classe dei Vf (con schema radicale (C)VNC-) era ancora vitale al punto da costituire un modello per altri verbi. *Donste* deve esser considerato allo stesso livello di *sprongde* (Vf che tende verso una sua regolarizzazione nei deboli con apofonia + desin. debole), per quanto il suo movimento sia opposto a quello di *sprongde*; cfr. anche *prak* (*/prekte*), *joeg* (*/jaagde*) da *preken* 'to preach', *jagen* 'cacciare', ted. *jagen* col pret. dialettale *jug*) e *esloeren* (*/eslierd* 'scivolato', PPa. da *slier'n:* vd. Lejosne 1982: 163): in tutti questi esempi siamo di fronte non a innovazioni lessicali più o meno arbitrarie ma a formazioni analogiche tendenti verso paradigmi regolari, evidentemente sentiti ancora come produttivi. Molti dialetti tedeschi conservano l'apofonia dei Vf meglio del tedesco standard (cfr. Schirmunski 1962: IV, 1): nessuna meraviglia quindi che Lejosne possa citare forme forti come *winke, gewunk* (Saarburg) per *winkte, gewinkt* o casi di conservazione di forme m. alto ted. quali *belle, geboll* (Saaralbe) per *bellte, gebellt* (:Lejosne 1982: 370sg.).

In conclusione il materiale linguistico attestato è una chiara prova della tendenza a costituire serie regolari di derivazione. L'estensione analogica poteva verificarsi in entrambe le direzioni (vd. specialmente la forte attrazione esercitata dalla III classe dei Vf). Coppie del tipo *korf/kerfde* e *erfde/urf* sono indicative della stessa tendenza a regolarizzare i paradigmi

Schema

	Ge-spann					urf (/erfde)	sprongde	sailed	
	song					murk (/merk(e)de)	told, sold	*bringed	
	verholen	I was	I flew	I done	I let	dolf (/delfde)	brought		
	strewn	I went	I sang	I spoke	I hit	korf (/kerfde)	brachte	*goed	
LESSICALIZZA-	Verwandte						dachte		MORFOLOGIZ-
ZIONE	←								→ ZAZIONE
	lessemi isolati (PPa.)	suppleti-vismo	3 gradi apofonici	2 gradi apofonici	1 grado apofonico	forte *e* debole debole *e* forte		apofonia + -d / -t	

e rappresentano verosimilmente il punto di contatto tra le due serie. Dal punto di vista diacronico, infine, la maggiore regolarità del tipo deb., con un minor numero di cambiamenti fonetici alteranti la forma, ha fatto sì che questo tipo vincesse sull'altro.

3. E' in realtà diventato impossibile prevedere quale vocale apparirà nelle alternanze di un Vf in una lingua come l'inglese, a causa delle troppe trasformazioni fonetiche intervenute rispetto alla forma che ci si attenderebbe dal punto di vista etimologico (cfr. n. 2). E tuttavia si è visto che paradigmi forti non solo sono presenti in tutte le lingue germaniche moderne (salvo l'afrikaans), ma che questi sono stati capaci fino ad epoca recente di attrarre analogicamente numerosi Vd. Come si spiega un simile stato di cose?

Nel momento in cui si ricercano le cause di un determinato sviluppo linguistico si è inevitabilmente condotti a prendere in esame anche la dimensione psicolinguistica. Ed infatti è possibile collocare le forme verbali che abbiamo sin qui esaminate lungo una scala i cui estremi sono rappresentati da 'grammatica' e 'lessico' e lungo la quale agiscono dinamicamente due principii, appunto 'grammaticalizzazione' e 'lessicalizzazione' dei quali è evidente la dimensione psicolinguistica.

La tipologia del fenomeno esaminato oscilla quindi per l'insieme dei verbi germanici fra questi due poli cosicché, per riprendere uno spunto suggestivo di Henry M. Hoenigswald, "the typology of [. . .] structural changes is [. . .] constrained by the typology of possible or probable structures" (Hoenigswald 1963: 45).

Uno dei risultati più interessanti dell'analisi condotta su vasta scala da Lejosne è che emerge dai varii sistemi dei Vf la tendenza ad una specie di polarizzazione delle vocali: vocali posteriori e mediane ([u], [o], [a]) che statisticamente sono le più frequenti nelle forme del preterito forte (in accordo coi paradigmi delle antiche 7 classi forti), sono sentite come tipiche del pret. (o passato che sia); vocali anteriori ([e], [i]) come tipiche del presente.[5] "Dans ces conditions le phénomène de polarisation [cioè vocali anteriori per il presente vs. vocali posteriori per il preterito] apparaît comme l'expression d'un refus de l'arbitraire, le maintien des oppositions fondamentales, la pérennisation d'un système bipolaire ou tripolaire [se includiamo anche il PPa.] servant de cadre général": Lejosne 1982: 969.[6]

Su questa base psicolinguistica comprendiamo meglio le neoformazioni come *spoke, broke* o come *urf, frug, gewunk*. E più in generale si può affermare che l'apofonia vocalica è sentita ancora come un segno privilegiato della flessione verbale (a differenza di quella nominale: vd. sùbito più sotto), sufficiente in sé a differenziare tempi (e modi!) anche in mancanza di altri morfemi (*:I give~I gave; ich komme~ich kam~ich käme*).

Ciò risulta particolarmente notevole, poiché l'introflessione è fenomeno sporadico nelle lingue germaniche (il tipo *tooth ~ teeth* non è più che un relitto in inglese, e anche in tedesco il tipo *Vater ~ Väter*, per quanto molto meglio rappresentato, non è più produttivo). Inoltre si deve tener conto della tendenza generale, propria di tutte le lingue germ., ad abbandonare la polimorfia dei morfemi: quest'ultima è caratteristica di lingue fortemente flessive (vd. p.es. il genit. sing. lat. *-ae, -ī, -is, -ūs*: 'morfemi sinonimici' secondo la terminologia di Skalička 1966); lo è molto meno per lingue analitiche quali sono diventate quelle germaniche. La persistenza dei Vf nel sistema verbale delle lingue germaniche, non solo come 'tokens' memorizzati ma anche come 'types' tuttora vitali, contraddice dunque la tendenza generale ('drift') dell'evoluzione tipologica delle lingue germaniche e prova 'e contrario' la centralità del sistema dei Vf.

L'opposizione comunemente accettata (e peraltro corretta) tra forme lessicalizzate, irregolari e imprevedibili da un lato e forme grammaticali, regolarmente producibili secondo schemi fissi, dall'altro – opposizione che abbiamo visto ora valida anche a livello psicolinguistico – deve essere precisata e sfumata: il sistema dei Vf non è solo un relitto di uno stato linguistico tramontato, ma è ancora una parte vitale della morfologia e le sue alternanze vocaliche sono sentite ancora come sufficientemente caratterizzanti (iconiche) in modo tale da proteggere i 'tokens' più ricorrenti dalla scomparsa.

Il polo a sinistra della scala non è occupato allora da forme verbali 'stricto sensu'; bensì da forme originariamente collegate al sistema verbale ma i cui legami con questo sono divenuti via via più deboli. Oltre ai sostantivi verbali (tipo *song, Trunk, Ge-spann*), queste forme sono rappresentate specialmente dai PPa. tipo afr. (*die*) *afgestorwenes* '(i) morti', (*die*) *aangesprokenes* '(gli) interpellati', ted. (*die*) *erkorenen* '(gli) eletti', (*die*) *Verwandten* '(i) parenti', neerl. *verholen* 'nascosto', a. isl. *flókenn* 'confuso', isl. *aldinn* 'vecchio', ingl. *cloven* (*-footed, hoof*) 'scisso', *shorn* 'privo', ecc.ecc. Simili forme isolate hanno spesso al loro fianco un corrispondente PPa.reg.: cfr. neerl. *verheeld*, ingl. *cleft, sheared.* Le forme forti (e quindi irregolari) appartengono di solito alla lingua letteraria, e spesso si tratta di arcaismi come nel caso di formule stereotipe (vd. p.es. afr. *met gebroke hart* vs. *'n gebreekte been*); ovvero il verbo è praticamente scomparso, come nel caso del ted. *kiesen.* Possiamo così trovare ancora PPa. forti mentre il pret. ha assunto forma debole: neerl. *weven* 'to weave', *weefde* ma ancora *geweven; wreken* 'to wreak', *wrekte* ma ancora *gewroken; scheiden* 'dividere, scheiden', *scheidde* ma ancora *gescheeën* 'divorziato', ecc.

4. Riassumendo: c'è una specie di equilibrio (dinamico, non statico) tra forme regolarmente producibili e predicibili che possiamo quindi considerare come generate dai meccanismi grammaticali da un lato e forme irregolari non-predicibili che sono immagazzinate come tali nella memoria dall'altro. Le forme dei Vf (e quelle dei Vd che si comportano come forti) sono scaglionate lungo l'asse di maggiore o minore grammaticalità. Possiamo considerare questo equilibrio come il risultato della dialettica fra i due principii opposti del minimo sforzo (che tende alla regolarità della grammatica) e la simbolicità espressiva (che tende verso forme non-motivate ma chiaramente riconoscibili, da immagazzinare nella memoria).[7]

Ritroviamo pertanto anche all'interno del sistema verbale la ben nota oscillazione tra i due poli che è alla base dei fenomeni di lessicalizzazione di forme appartenenti alla morfologia e viceversa di morfologizzazione di lessemi (caso, p.es., del gr. *phílos* 'amico' – aggettivo e anche sostantivo, ma originariamente 'proprio', aggett. pronominale – e inversamente del celt. *priātus* (< lat. *prīvātus*) 'proprio' (vd. gall. *priawt*) ma originariamente 'sposo' (vd. bret. *priet* 'sposo': cfr. Markey 1985).

Fra i due poli, 'grammatica' e 'lessico', c'è una scala con possibilità di spostamenti (verso destra e verso sinistra) cosicché è giusto parlare di equilibrio dinamico e, piuttosto che di 'lessico' e 'grammatica', di processi di lessicalizzazione e grammaticalizzazione.

Note

[1] Una descrizione molto accurata dello 'status quaestionis' nella ricerca psicolinguistica è costituita dalla 'Thèse pour le Doctorat d'Etat' di Jean-Claude Lejosne (1982). Il presente articolo deve molto alla lettura di questa 'Thèse'.

[2] Troppi accidenti dell'evoluzione fonetica hanno invero contribuito ad alterare la chiara regolarità del vecchio sistema apofonico germanico. Si prenda p.es. un verbo quale il fris. ***tsjen*** 'trascinare' (< germ. ****teuhan-***, del got. ***tiuhan*** ecc.). Nella forma attuale troviamo: assibilazione di ***t-*** nell'affricata ***ts-*** davanti a ***-j-; eu*** > ***iu*** > ***i, j; -h-*** > ∅; ma questo ***-h*** si mantiene in posizione finale: pret. sing. ***teach***; nel plur. ***teagen*** sono visibili ancora gli effetti dell' 'alternanza grammaticale' (Legge di Verner); ma il PPa. ***tein*** non ne mostra alcuna traccia e l'apofonia non è in ***e(a)***, come normalmente ci attenderemmo per un verbo della II classe (cfr. ***frieze/fear/ferzen*** 'to freeze'). Gli esempi di alterazione rispetto ai paradigmi originarii potrebbero facilmente moltiplicarsi.

[3] Si può pensare che in alcuni casi le forme forti si siano sviluppate sulla base della corrispondente debole quando questa era foneticamente assai simile: vd. p.es. ***sande*** [zǎndə] o l'inserzione di ***-d-*** nel tipo ***beliën***. Ma nella gran maggioranza dei casi (vd. il già cit. ***zwoer/zweerde***) non c'è possibilità di passare foneticamente dall'una all'altra forma.

[4] P.es. ***jog*** da ***jage*** 'cacciare, jagen', che però è un prestito dal basso ted. (cfr. neerl. ***joeg***, ted. dial. ***jug***), ***skauv*** da ***skyve*** 'spingere, schieben': vd. O. Beito 1970: § 380sg.

[5] Lejosne (1982: 966) riporta i risultati dell'analisi statistica condotta sul ted. da E. Mater (1966–72: V 108–112):
Vf pres.: ***e*** 50, ***ei*** 41, ***i*** 34, ***ä*** 5, ***ie*** 24; ***u*** 1, ***o*** 2;
pret.: ***a*** 62, ***o*** 50, ***u*** 14 (vs. 53 forme con vocale anteriore).

[6] Lejosne cita anche le statistiche che mostrano come la quantità d'informazione fornita da vocali posteriori in inglese sia decisamente più alta rispetto a quella delle vocali anteriori (3,14 vs. 1, 36 bits: vd. Hood Roberts 1965: 40). Risultati analoghi per il ted.: voc. post. 2,63 bits vs. voc. anter. 1, 54 bits (vd. Meier 1964).

[7] Si veda l'idea di Lyons (1974: 90) di considerare la lingua come un sistema omeostatico di autoregolamentazione basato sul principio del minimo sforzo, che tende a raggiungere l'efficienza massima del sistema stesso, e il desiderio di essere compresi chiaramente. Non è di certo una fortuita coincidenza che la stessa situazione di rilevanti irregolarità nei 'tokens' più frequenti la si ritrovi in altri sottoinsiemi della grammatica (si pensi p.es. ai comparativi tipo ***better, best***, ai pronomi). Per il ted. vd. Augst 1975: 274, nota 6.

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Sonderfall Griechisch?

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1. Bei der Beschäftigung mit der griechischen Sprache und ihrer Geschichte wird man das Gefühl nicht los, daß hier manches Einmalige vorliegt, etwas, das anders ist, als wir es von anderen Sprachen her kennen, oder sich wenigstens in einer besonders eindrücklichen Weise manifestiert. Man ist daher gerne geneigt, von einem Sonderfall zu sprechen. Nun ist jede Sprache etwas Besonderes und Einmaliges, gewissermaßen ein Sonderfall. Aber wahrscheinlich ist es die griechische in einem stärkeren Maße als manche andere.

Für die historische Sprachbetrachtung ist es singulär, daß wir hier die Entwicklung von den mykenischen Tontafeln des 14. und 13. Jahrhunderts bis heute, also über eine Zeitspanne von etwa 3300 oder 3400 Jahren, unmittelbar verfolgen können. Verschiedene andere Sprachen sind zwar schon früher schriftlich fixiert worden; doch sind sie im Laufe der Zeit ausgestorben. Die arischen Sprachelemente im churritischen Mitannireich sind, so wertvoll und interessant sie auch sind, noch keine wirklichen Texte. Im Falle des Chinesischen erschwert das völlig andersartige Schriftsystem, sich ein Bild von den Veränderungen der Sprache zu machen.

Bereits die mykenischen Texte lassen eine Differenzierung des Griechischen in verschiedene Dialekte erkennen, die dann im ersten vorchristlichen Jahrtausend dank zahlreichen Dialektinschriften in einer einmaligen Weise greifbar wird. Freilich hat von diesen altgriechischen Dialekten nur sehr weniges das Ende des Altertums überlebt; von vereinzelten Ausnahmen abgesehen, gehen die modernen Dialekte nämlich alle auf die Koine zurück, die sich seit der hellenistischen Zeit aus dem attischen Dialekt entwickelte. Die neuiranischen Sprachen und Dialekte setzen mehr alte Dialektunterschiede fort, doch ist das Altiranische nicht so früh und lange nicht so gut bezeugt.

2. Zweierlei fällt beim Griechischen auf: erstens, daß bereits das Altgriechische, nach dem Hethitischen (bzw. Anatolischen) die frühestbezeugte indogermanische Sprache, sich durch verschiedene Merkmale deutlich von den anderen Sprachen abhebt, und zweitens, daß das Neugriechische diese Merkmale mit einer auffallenden Treue nicht nur bewahrt, sondern vielfach sogar weiter entwickelt hat.

2.1. Das gilt vor allem für das Verbum. Das Altgriechische ist eine typische Aspektsprache, indem hier scharf zwischen durativem (oder linearem, präsentischem) und aoristischem (oder punktuellem) Aspekt unterschieden wird[1], wozu als dritter Aspekt das Perfekt (zunächst ausschließlich Zustandsperfekt[2]) kommt, das freilich eine eher periphere Rolle spielt. Wir haben allen Grund anzunehmen, daß dieser Aspektcharakter aus der Grundspra-

che ererbt ist[3]. Wenn man nur die Formen betrachtet, ist die Übereinstimmung mit dem Indoiranischen sehr groß; doch verlieren Aorist und Perfekt im Altindischen schon früh den Aspektcharakter und werden immer mehr zu Vergangenheitstempora. Ähnliches gilt auch fürs Iranische. Im Latein fällt der Aorist mit dem alten Perfekt zu einer neuen Kategorie zusammen, die man ebenfalls Perfekt nennt, die aber in der Regel nicht einen Zustand, sondern den Abschluß der Handlung oder deren Vorzeitigkeit ausdrückt. Eine Temporalisierung der Aspektkategorien läßt sich auch sonst beobachten. Im Gegensatz dazu hat das Griechische bis heute nicht nur den Gegensatz Durativ/Aorist bewahrt[4], sondern hat ihn sogar noch ausgebaut, indem es ihn auch aufs Futur ausgedehnt hat.

Ausgedrückt werden die Aspekte im Neugriechischen wie im Altgriechischen in erster Linie durch besondere Stammformen, z.B. *γράφω* 'ich schreibe' – Aor. *ἔγραψα*, *δένω* 'ich binde' – *ἔδεσα*, *μένω* 'ich bleibe' – *ἔμεινα*, *μαθαίνω* 'ich lerne' – *ἔμαθα*, *φοβοῦμαι* 'ich fürchte mich' – *φοβήθηκα*[5]. Wie diese Beispiele zeigen, ist nicht nur das Prinzip als solches bewahrt, sondern es sind in vielen Fällen sogar die einzelnen Formen praktisch unverändert geblieben. Die Stammformen bleiben also eindeutig Aspektstämme, während sie sich in den andern Sprachen im allgemeinen zu Tempusstämmen entwickeln.

2.2. Dagegen werden die eigentlichen Tempora Gegenwart (Präsens) und Vergangenheit (Präteritum) im Griechischen durch ganz andere Mittel ausgedrückt, nämlich durch besondere sog. primäre, bzw. sekundäre Personenendungen und zweitens (bei der Vergangenheit) durch ein zusätzliches Präfix, das sog. Augment[6]. Beides gilt auch fürs Neugriechische, doch ist das Augment in der Regel nur dann erhalten, wenn es akzentuiert ist. Dafür ist das Prinzip der besonderen Endungen auf alle Personen ausgedehnt worden, z.B.

Präs. Sing.	*δένω*,	*δένεις*,	*δένει*,	Plur.	*δένουμε*,	*δένετε*,	*δένουν*
Impf.	*ἔδενα*,	*ἔδενες*,	*ἔδενε*,		*δέναμε*,	*δένατε*,	*ἔδεναν*
Aorist	*ἔδεσα*,	*ἔδεσες*,	*ἔδεσε*,		*δέσαμε*,	*δέσατε*,	*ἔδεσαν*.

Die Unterscheidung von primären und sekundären Personalendungen ist also im Neugriechischen – im Gegensatz zur Entwicklung in anderen Sprachen – nicht nur beibehalten, sondern weiter ausgebaut worden[7].

Zu bemerken ist noch, daß die primären Personalendungen wie im Altgriechischen auch für den Konjunktiv verwendet werden, der beim Durativ mit dem Indikativ Präsens zusammenfällt, aber beim Aorist besondere Formen hat. Das eigentliche Moduszeichen ist aber eine Partikel, meist *νά* (< *ἵνα*). Diese Form ist unter anderem deshalb wichtig, weil sie auch zur Umschreibung des fehlenden Infinitivs dient. Mit der Partikel *θά* (< *θέλω ἵνα*) verbunden bildet der Konjunktiv das neue Futur, z.B. durativ *θὰ γράφω*, aoristisch *θὰ γράψω*.

2.3. Als weiteres Erbstück kommt dazu, daß sogar noch im Neugriechischen durch die Personalendungen auch die Diathesen ausgedrückt werden. Außer dem Aktiv gibt es ein Mediopassiv, das beim Durativ das alte Medium und beim Aorist das alte Passiv fortsetzt. Die Bedeutung ist teils passiv, z.B. *δένομαι* 'ich werde gebunden' (Aor. *δέθηκα*), teils medial, z.B. *λούζομαι* 'ich bade (mich selbst)'; es gibt auch zahlreiche Deponentien, z.B. *φοβοῦμαι* 'ich fürchte mich' (s. oben).

2.4. Man beachte also die große Aussagekraft nicht nur der altgriechischen, sondern ganz besonders auch der neugriechischen Personalendungen, die hierin sehr an die hethitischen erinnern: Außer der Angabe von Personen und Numerus (also des Subjekts) drücken sie

noch Tempus (bzw. Modus) und Diathese aus. Dagegen werden die Aspekte grundsätzlich durch besondere Stammformen ausgedrückt. Das Neugriechische hat aber nicht nur Präsens und Aorist, sondern auch das indogermanische Imperfekt nach Form und Inhalt im wesentlichen unverändert bewahrt (zum neugriechischen Verbum vgl. Seiler 1952).

Während andere Sprachen den Konjunktiv schon früh aufgegeben haben, es sei denn, daß er – wie im Latein oder Altpersischen – als Futur eine neue temporale Funktion bekam, ist dieser Modus im Griechischen bis heute lebendig geblieben. Anderseits ist der Optativ, der sich sonst viel besser als der Konjunktiv hielt, hier verloren gegangen. Wie das Perfekt (s. Anm. 4) verschwindet der Optativ weitgehend bereits in der Koine der Kaiserzeit (Schwyzer-Debrunner 1950: 337f.). Auch der auffälligste Verlust, nämlich der des Infinitivs, den das Griechische bekanntlich mit den anderen Balkansprachen teilt, bahnt sich in dieser gleichen Zeit an[8].

3.1. Auch das Nomen zeigt heute verschiedene altertümliche Züge. Zwar ist der Dativ aufgegeben worden, auch das infolge einer Entwicklung, die sich im späten Altertum abzeichnet[9]. Sonst sind alle Kasus des Altgriechischen erhalten, sogar der Vokativ, und ein Paradigma wie

Sing. Nom.	ἀδερφός 'Bruder',	Akk. ἀδερφό,	Gen. ἀδερφοῦ,	Vok. ἀδερφέ
Plur. Nom.	ἀδερφοί,	Akk. ἀδερφούς,	Gen. ἀδερφῶν	

stimmt nicht nur mit dem altgriechischen völlig überein, sondern erinnert nicht bloß zufällig etwa an litauisch *draũgas* 'Freund', Akk. *draũgą*, Gen. *draũgo*, Vok. *draugè*, Plur. *draugaĩ*, Akk. *draugùs*, Gen. *draugũ*, nur daß im Litauischen drei weitere Kasus, Dativ, Instrumental und Lokativ dazukommen. Dafür ist das Neutrum sehr gut erhalten, und zwar nicht nur bei der *o*-Deklination, sondern auch in Paradigmen wie τὸ μέρος 'Teil' (Gen. μέρους, Plur. μέρη, μερῶν) und ὄνομα 'Name' (Gen. ὀνόματος, Plur. ὀνόματα, ὀνομάτων)[10]. Die Feminina und Maskulina der ersten Deklination sind freilich stärker umgebildet worden und sind mit denen der dritten Deklination zusammengefallen.

3.2. Eine besondere Altertümlichkeit ist aber, daß der Typus der *o*-stämmigen Feminina wenigstens in Resten erhalten ist. Die hierher gehörenden Wörter stammen freilich offenbar aus der schriftsprachlichen Tradition, wie z.B. ἡ ἔξοδος 'Ausgang', ἡ εἴσοδος 'Eingang, Eintritt' u.ä., ebenso die restituierten antiken Ortsnamen wie ἡ Χίος. Volkssprachlich sind sie nach dem Vorbild der übrigen Feminina umgestaltet worden, z.B. ἡ παρθένο 'Jungfrau', Akk. τὴν παρθένο, Gen. τῆς παρθένος, Plur. οἱ παρθένες, Akk. τὶς παρθένες, Gen. τῶν παρθένων[11]. In diesem Punkt ist das Neugriechische offenbar altertümlicher als sogar das Altindische, das – wie die meisten anderen Sprachen – solche Feminina in die *ā*-Stämme übergeführt hat, z.B. *snuṣā́* 'Schwiegertocher' statt **snusós*, gr. νυός[12].

4. Zusammenfassend darf man sagen, daß das Neugriechische zu den altertümlichsten unter den heutigen indogermanischen Sprachen gehört. Zwar ist der Wortschatz bereits im Altgriechischen stark umgestaltet; aber auf dem Gebiet der Morphologie übertrifft es in mancher Hinsicht alle anderen. Auch bei den Lauten ist manches bemerkenswert, so die Erhaltung des auslautenden *-s* oder die nahezu unveränderte Erhaltung, jedenfalls was die Qualität betrifft, der alten Kurzvokale, die übrigens zum Teil auch die verschiedenen Laryngale fortsetzen[13].

5. Es stellt sich die Frage, wie diese und ähnliche Besonderheiten des Griechischen allgemein und des Neugriechischen im besonderen zu erklären sind. Daß es sich um zufällige, von einander unabhängige Einzelerscheinungen handelt, wird man wohl ausschließen dürfen. Doch werden vermutlich verschiedene Ursachen zusammengewirkt haben.

5.1. Zunächst ist die Ähnlichkeit zwischen dem Neugriechischen und dem Altgriechischen zu betrachten, welche, was die grammatische Struktur betrifft, bedeutend größer als etwa die zwischen dem Latein und den modernen romanischen Sprachen ist. Außer den bereits besprochenen Erscheinungen kann genannt werden, daß der neugriechische Lautstand im großen und ganzen jenem entspricht, den das Griechische am Ende des Altertums erreicht hatte. Erhalten ist vor allem, was morphologisch sehr wichtig ist, das auslautende -ς, und wenn gerade in den vorangehenden Jahrhunderten die Diphthonge und die Langvokale mannigfaltige Veränderungen erfahren hatten, so ist doch festzuhalten, daß normalerweise Vokale als solche erhalten bleiben und nicht etwa schwinden, so daß die historische Schreibweise auch bei veränderter Aussprache beibehalten werden konnte.

Sicher hat der überragende Einfluß der Kirche und des Klerus, der gerade in der Zeit der Türkenherrschaft besonders wichtig war, entscheidend dazu beigetragen, daß die in frühchristlicher Zeit entwickelte Form des Griechischen sich in mancher Hinsicht unverändert erhalten hat[14]. Damit erklärt sich sicher vieles, u.a. die Erhaltung der altertümlichen Paradigmen beim Neutrum, nicht aber die Schaffung neuer Paradigmen bei den Maskulina und Feminina der ersten und dritten Deklination. Noch weniger verständlich ist, wieso das Verbum bei Erhaltung vieler Einzelformen und neben zahlreichen größeren und kleineren Umgestaltungen ein neues Gesamtsystem von einer imponierenden Geschlossenheit erreichen konnte.

5.2. Offenbar setzen sich im Neugriechischen Entwicklungstendenzen fort, deren Wurzeln bedeutend älter sind und zum Teil bis in die frühgriechische Zeit zurückreichen. Das Mykenische zeigt uns, daß die Nominalflexion im zweiten Jahrtausend v.Chr. der grundsprachlichen noch merklich näher steht als in klassischer Zeit[15]: erhalten war noch als selbständiger Kasus der Instrumental, auch waren verschiedene andere Altertümlichkeiten bewahrt. Doch war z.B. bei den Nomina agentis auf *-τήρ* bereits eine einzige Ablautstufe verallgemeinert worden, und die Neutra auf *-μα* (< **-mn̥*) hatten in den übrigen Formen bereits den Stamm *-ματ-* (bzw. mit einer fürs Mykenische typischen Lautentwicklung *-mot-*)[16]. Schon in vormykenischer Zeit hatte sich also die Tendenz zu größtmöglicher Klarheit und Einfachheit wenigstens bei den häufigeren Paradigmen durchgesetzt. Diese Tendenz zeigt sich auch später, indem etwa die Dative auf *-oi^{h}i* und *-ā^{h}i* (z.B. *te-o-i /the^{h}oi^{h}i/* 'den Göttern', *ku-na-ke-ta-i /kunägetā^{h}i* 'den Jägern'), also mit lautgesetzlichem Wandel des intervokalischen *s* zu *h*, nachmykenisch in Analogie zu den Formen der dritten Deklination zu *-οισι* und *-āσι* (so noch altattisch, z.B. *ταμίασι*) umgestaltet wurden. Wichtig war vor allem die Reduktion der Kasus auf fünf, wie wir sie in klassischer Zeit bei allen griechischen Dialekten finden. Auf der anderen Seite zerstörte die Kontraktion aneinander stoßender Vokale in der ersten Hälfte des ersten Jahrtausends die bisherige Klarheit und Einheitlichkeit der Paradigmen, was dann vor allem in der Koine zu mannigfaltigen Umbildungen führte, die sich zum Teil sogar in den neugriechischen Paradigmen fortsetzen.

Die Entwicklung vollzieht sich also in kleinen und kleinsten Schritten ohne eigentliche Bruchstellen, sozusagen organisch bei gleichbleibender Tendenz, nämlich Schaffung und Erhaltung einfacher Paradigmen. Da seit dem Ende des Altertums nur geringfügige lautliche

Veränderungen eintraten, bestand nur wenig Anlaß, undeutlich gewordene Paradigmen umzugestalten[17].

5.3. Über das mykenische Verbum sind wir leider viel schlechter orientiert als über das Nomen. Doch finden wir auch hier Formen, welche denen der indogermanischen Grundsprache näher stehen als denen des klassischen Griechisch, z.B. *ki-ti-je-si /ktijensi/* 'sie bebauen' = ai. *kṣiyánti*, ohne direkte Fortsetzer im späteren Griechisch. Anderseits ist aber manches typisch Griechische bereits voll ausgebildet. Man hat schöne Belege für die sog. attische Reduplikation, z.B. *a-ra-ru-ja*, wie hom. *ἀραρυῖα* (Part.Perf.fem.) 'gefügt' oder *a-ra-ro-mo-te-me-na /ararmotmenā/* 'zusammengesetzt (von einem Wagen)', Part.Perf.Med. zu einem Verbum, das im späteren *ἁρμόζω -όττω* 'zusammenfügen' weiterlebt. Die größten Folgen hatte wohl die Umbildung des ererbten *s*-Aoristes zum griechischen *σα*-Aorist, z.B. *de-ka-sa-to /deksato/* = *(ἐ)δέξατο* 'er bekam'. Denn nun war statt eines Paradigmas mit komplizierten Konsonantengruppen ein wesentlich einfacheres Paradigma gewonnen, das eindeutig aoristisch war[18] und außerordentlich produktiv werden konnte. Man darf annehmen, daß diese Umbildung wesentlich dazu beigetragen hat, daß das Griechische den Charakter einer Aspektsprache behalten und sogar festigen konnte.

Dieser *σα*-Aorist ist also bereits vormykenisch. Die Formen mit intervokalischem, entgegen den Lautgesetzen erhaltenem *s*, z.B. myk. *e-ra-se /elase/* 'er trieb' (= *ἤλασε*), zeigen, daß sie offenbar jünger sind als der Wandel $s > h$[19]. Da dieser so gut wie sicher älter als die Einwanderung der indogermanischen Vorfahren der späteren Griechen in Griechenland ist, die man um 2000 v.Chr. datiert (Risch 1979: 273f.), wird man die Schaffung des *σα*-Aoristes etwa in die erste Hälfte des zweiten Jahrtausends ansetzen können. Zusammen mit den meist sehr deutlichen Durativ-(Präsens-)Stämmen erlaubten nun die ebenso deutlichen Aoriststämme eine klare Unterscheidung der Aspekte und zwar vor allem durch besondere Stammformen (Aspektstämme). Daher konnte zur Unterscheidung der eigentlichen Tempora Gegenwart und Vergangenheit das Prinzip der primären und sekundären Personalendungen nicht nur bleiben, sondern sogar ausgebaut werden (s. 2.2.). Das Nebeneinander beider Prinzipien, der Stammformen für die Unterscheidung der Aspekte und der besonderen Endungen zum Ausdruck von Gegenwart und Vergangenheit blieb jedenfalls erhalten und damit u.a. auch das alte Imperfekt.

6. Aus dieser notgedrungen sehr summarischen Betrachtung ergibt sich, daß offenbar verschiedene Momente zur eigenartigen Stellung des Griechischen und speziell des Neugriechischen geführt haben. Wichtig ist, daß schon sehr früh ein deutliches Streben nach klaren Paradigmen faßbar wird, welche im Laufe der Zeit zwar immer wieder umgebaut werden können, aber in ihrer Grundstruktur nur wenig verändert werden. Dazu kommt, daß sich das Griechische in lautlicher Hinsicht seit dem Ende des Altertums nur wenig verändert hat, was wohl – wenigstens teilweise – auf den Einfluß der Kirche und des Klerus zurückzuführen ist, dem auf alle Fälle die Erhaltung mancher morphologischer Altertümlichkeiten zu verdanken ist.

Anmerkungen

[1] Ich meide die leider sehr verbreiteten Ausdrücke imperfektiv und perfektiv, da sie mißverständlich sind und immer wieder zu irrigen Vorstellungen führen. Natürlich beruhen sie irgendwie darauf, daß z.B. russ. *ja pisál* mit dem lat. Imperfekt *scrībēbam* und *ja napisál* mit dem Perfekt *scrīpsī* wiedergegeben werden können. Aber mit der Frage, ob die Handlung zum Abschluß kommt oder nicht, hat diese Aspektunterscheidung an sich nicht viel zu tun. Im Übrigen steht im Latein Perfekt (genauer Praesens perfectum) als Gegensatz zu Präsens (genauer Praesens imperfectum) und nicht, jedenfalls zunächst nicht, zu Imperfekt (genauer Praeteritum imperfectum).

[2] Nach der sehr interessanten Theorie, welche W. Cowgill (1979: 25–39) vorlegt, hätte sich das Zustandsperfekt aus der noch älteren Kategorie "nominal verbs" entwickelt, also aufs Griechische übertragen z.B. κέκραγε, eigtl. 'er ist ein Schreier', was der tatsächlichen Bedeutung sogar sehr nahe kommt.

[3] Vor allem die Tatsache, daß bestimmte Verbalwurzeln nur als Durativ (z.B. **h₁es-*) oder nur als Aorist (z.B. **bhewh₂-/bhuh₂-*), etwa auch als Aorist und Perfekt (z.B. **weid-*) verwendet werden, spricht m.E. eindeutig dafür, daß eine zunächst semantisch bedingte lexikalische Unterscheidung nachträglich, aber bereits grundsprachlich, als Aspekt grammatikalisiert wurde. Die Tendenz, bei jedem Verbum möglichst alle drei Aspekte zu bilden, setzt sich evident erst nach und nach (und sehr unvollkommen) durch.

[4] Das alte Perfekt ist im Neugriechischen verschwunden, und zwar im wesentlichen bereits im späten Altertum, s. E. Schwyzer (1939: 779), P. Chantraine (1961: 199ff.). Neu dazugekommen ist aber ein periphrastisches possessives Perfekt vom romanischen und germanischen Typus *habeo ligatum*, nämlich ἔχω δεμένο (< δεδεμένον) und ἔχω δέσει (δέσει eigtl. Inf. Aor.). Doch spielt es im Neugriechischen bei weitem nicht die Rolle wie in den romanischen und germanischen Sprachen.

[5] Wenn nichts anderes vermerkt wird, stütze ich mich fürs Neugriechische auf die offizielle Νεοελληνικὴ γραμματική (1976), die auf der Μικρὴ Νεοελληνικὴ γραμματική von M. Triandaphyllidis basiert. Spätere Veränderungen, z.B. bei der Schreibung der Akzente, sind nicht mehr berücksichtigt.

[6] Ursprünglich eine selbständige Partikel, vgl. Risch (1975: 476).

[7] Es sind also im Prinzip die Endungen des sigmatischen Aoristes zunächst auf den starken Aorist und später auch auf das Imperfekt übertragen worden, s. Anm. 18.

[8] Vgl. etwa Burguière (1960). Doch ist zu beachten, daß im pontischen Dialekt der Infinitiv, wenn auch in reduzierter Verwendung, bis heute geblieben ist.

[9] Vgl. Humbert (1930), in allgemeinerem Rahmen Seiler (1958: 41–67). Als Zeugnis für den Zusammenfall von Genetiv und Dativ verdienen drei christliche Grafitti aus Kyrene Beachtung: SEG 31. 1578 (a) Κύριε, βοήθησον τοῦ Ἰανουαρίῳ, (b) Βοήθησον, Κύριε, τοῦ ῥαββί, (c) (= SEG 9. 188) Κύριε, βοήθησον Ἀναστασίῳ τῷ ἔχοντος τὰς περεστερὰς (sic!). Man hat den Eindruck, daß hier der kyrenäische Dialekt nachwirkt, bei dem sich der Genetiv auf -ω besonders zäh gehalten hat. Wegen des lautgesetzlichen Zusammenfalls des Dativs auf -ωι mit dem Genetiv lag es nahe, auch sonst zwischen diesen beiden Kasus nicht zu unterscheiden.

[10] Dem alten Typus ὄνομα haben sich die Abstrakta auf -σιμο angeschlossen, z.B. τὸ δέσιμο 'das Binden': δεσίματος, δεσίματα, δεσιμάτων usw.

[11] Der Typus ἡ παρθένο fehlt in der Νεοελληνικὴ γραμματική (s. Anm. 5), findet sich jedoch bei Mirambel (1939: 56f.).

[12] Außer dem Griechischen kennt noch das Latein einige Feminina nach der *o*-Deklination, z.B. *humus* 'Erdboden', *alvus* 'Bauch', Baumnamen wie *fāgus* 'Buche' usw., ähnlich auch im Oskischen, Vetter nr. 11 *trífibúm ekak . . . úpsannam deded* 'er hat dieses Gebäude machen lassen', jedoch im Gegensatz zum Altgriechischen keine Adjektive auf *-us* zweier Endungen; auch ist *nurus* 'Schwiegertochter' nach dem Vorbild von *socrus* 'Schwiegermutter' in die *u*-Deklination übergeführt worden. Das älteste Latein hatte noch *lupus fēmina* 'Wölfin', ebenso *agnus fēmina* 'weibliches Lamm' und *porcus fēmina* 'weibliches Ferkel' (also wie griech. ὁ und ἡ ἵππος), was bereits im Altlatein durch *lupa, agna* und *porca* ersetzt wurde. Man kann sagen, daß das Neugriechische hinsichtlich der Erhaltung der *o*-stämmigen Feminina ungefähr auf der Stufe des Lateins steht.

[13] Z.B. ε in θέτω 'ich setze, stelle' als Reflex von h_1, α in πατέρας 'Vater' als Reflex von h_2, ο in βοτάνι 'Kraut' als Reflex von h_3.

[14] Dialekte, welche sich unabhängiger entwickeln konnten, zeigen tatsächlich bedeutend größere Veränderungen, z.B. im Tsakonischen καλέ = καλός und καλόν, d.h. Schwund des auslautenden ς, ferner vielfach Wandel ο > ε, vgl. auch τὸ σέρε (σέρι) < θέρος 'Erntezeit'.

[15] Wie die Nominalflexion in der Grundsprache genau aussah, können wir hier offen lassen. Doch möchte ich bemerken, daß ich die verbreitete Ansicht, daß das Altindische das alte Kasussystem unverändert erhalten habe, nicht teilen kann. Vgl. dazu meine Ausführungen (1980: 259ff.).

[16] Ausführlicher darüber, sowie über die im folgenden genannten Erscheinungen des Frühgriechischen Risch (1984: 165–187, besonders 174f., 176, 179ff.).

[17] Der erst im Frühmittelalter erfolgte Wandel von *υ* /ü/ zu /i/ führte z.B. zum Zusammenfall der Paradigmen βαθύς 'tief' und δεξής (<δεξιός) 'rechts'.

[18] Die Personalendungen mit dem Quasi-Themavokal *α* wurden später auch auf den thematischen (starken) Aorist und nicht viel später auch auf das Imperfekt übertragen, z.B. im NT ἔβαλαν 'sie warfen' usw., sogar (als variae lectiones) εἴχαμεν *-αν* usw., s. das neugriechische Paradigma unter 2.2.

[19] Dagegen ist das *s* im dazu gehörenden Futur (ursprünglich Konjunktiv Aorist), wo es seit jeher intervokalisch war, lautgesetzlich geschwunden, z.B. ἐλάω 'ich werde treiben' (mykenisch sind die Partizipien *da-ma-o-te* und *de-me-o-te* /*-ontes*/ zu den Wurzeln **demh*$_2$- 'zähmen' und **demh*$_1$- 'bauen' bezeugt).

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Zur avestischen Kompositionslehre: *aš.*- 'groß'

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1. Das jav. Kompositionsanfangsglied (im folgenden KA) *aš.*- kommt überwiegend in Possessivkomposita (PK) vor und wird in diesen von Bartholomae, Wb. (ähnlich Duchesne-Guillemin, Les composés de l'Avesta, 1936, 188f.) durch die Adjektiva 'groß', 'reichlich', 'stark', 'tüchtig', 'viel' wiedergegeben (z.B. *aš.bāzu-* 'mit tüchtigen, starken Vorderbeinen'). Außerdem finden Bartholomae, dessen Übersetzungen ich hier zunächst gebe, und Duchesne-Guillemin 62, 76, 91f., 121, 131 *aš.*- vereinzelt in synthetischen Nomina agentis, z.B. *aš.bərət-* 'viel, reichlich bringend', synthet. Nomina actionis, z.B. *aš.yešti-* 'reichliches Opfern', sowie in adjektivischen Determinativkomposita (DK), z.B. *aš.paouruua-* 'der weitaus voranstehende, erste'.

Seit Bartholomae, ZDMG 50 (1896) 703[2], IF 9 (1898) 282f. und Wb. 229 sieht man in *aš.*- für gewöhnlich eine Bildung mit adverbiellem *-*s* zu idg. **meĝ-* 'groß'[1]. Als Bedeutung dieses selbständig nicht belegten Adverbs setzt Bartholomae 'sehr, viel' an. Weiters vergleicht er das nur gav. bezeugte Adv. *maš* < **meĝ-s* etwa 'sehr' Y. 32.3, 34.9[2] und führt als Parallele für die Ablautdifferenz gr. *ἀγα-* : *μέγα* an.

Ich versuche im folgenden, die synchronen Gebrauchsweisen von *aš.*- herauszuarbeiten und nach Möglichkeit zu erklären. Anhangsweise gebe ich eine neue morphologische Einordnung von *aš.*-.

2. Ich beginne mit den PK. Es ergibt sich zweifelsfrei, daß *aš.*- semantisch nichts anderes als die Kompositionsform von *mazāṇt-* bzw. *maz-* 'groß' (und dem mit diesen in der Bedeutung zusammengefallenen *mas-*)[3] darstellt. In einem Fall steht *aš.*- in direktem Gegensatz zu *kasu-* 'klein' (+*aš.dāna-* : +*kasu.dāna-*), für mehrere Endglieder (KE) sind freie Verbindungen mit *mazāṇt-* belegt. Besonders wichtig aber ist, daß *mazāṇt-* selber nie als KA vorkommt, sich also mit *aš.*- in komplementärer Distribution befindet. Damit erhält *aš.*- einen wohldefinierten Platz innerhalb der avestischen Kompositionsmorphologie.

2.1. *aš.dānu-* (wohl +*aš.dāna-*) 'mit großen Körnern' Yt. 8.29: *us vō apąm aδauuō/apaiti. ərətå jasāṇti/+aš.dānanąmca yauuanąm/+kasu.dānanąmca*[4] *vāstranąm* 'Die Gerinne eurer Wasser werden ungehindert herauskommen zum[5] großkörnigen Getreide und zum kleinkörnigen Gras'[6]. Das handschriftliche *dānunąmca* (und damit ein Stammansatz **dānu-* 'Korn') ist wegen jav. *dānō.karša-* 'Körner schleppend' und der verwandten Formen mpers. *d'n'*, ved. *dhā́nā-*, die av. **dānā-* erwarten lassen, suspekt und beruht vielleicht auf Einfluß des GPl. *dānunąm* 'der Dānus' Yt. 13.38 *ter.*

2.2. *aš.bāzu-* 'mit großen Vorderbeinen' Yt. 14.12: *yå uštrō pāiti* +*vaδairiš/aš.bāzāuš stuui.kaofō* '(die Kamelstuten,) die der brünftige Kamelhengst beschützt, der große Vorderbeine und starke Höcker hat'.

2.3. *aš.ama-* 'von großer Kraft, sehr kräftig' Yt. 13.107 u. Par.[7]: *kainīnō kəhrpa srīraiiå/ aš.amaiiå huraoδaiiå* 'in der Gestalt eines schönen, sehr kräftigen, gutgewachsenen Mädchens'. Vgl. die freie Phrase *mazišta amą* 'die größten Kräfte' Y. 13.3.

2.4. *aš.aojah-* 'von großer Stärke, Macht', im Jav. stets Beiwort von *druj-*. Y. 9.8 u. Par.: *yō janaṯ ažīm dahākəm/.../* +*aš.aojaŋhəm daēuuīm drujim/aγəm gaēθāuuiiō druuaṇtəm/ yąm* +*aš.aojastəmąm drujim/fraca kərəṇtaṯ aŋrō mainiiuš* 'der den Aži Dahāka erschlug, ... ihn, die sehr mächtige Daēuuī Druj, den für die Welt Üblen, den Trughaften, den Aŋra Mainiiu als die allermächtigste Druj hervorbrachte'. Y. 57.15: *yō jaṇta* +*daēuuiiå drujō/ aš.aojaŋhō* +*ahūm.mə… 'der die sehr mächtige, lebenszerstörende Daēuuī Druj erschlug'. Zu gav. +*aš.aojå* s. 4.1, zu *aš.aojastara-, aš.aojišta-* s.5.1. Vgl. noch die freie Phrase *mazištəm aojō* 'die größte Kraft' Yt. 14.12.

2.5. +*aš.hunara-* 'große Fertigkeit, Tüchtigkeit besitzend' Yt. 10.25: +*aš.hunarəm*[8] *tanu. mąθrəm/bāzuš.aojaŋhəm raθaēštąm*[9] '(den Miθra,) der große Tüchtigkeit besitzt, der den Mantra in sich hat, den starkarmigen Krieger'. Bartholomae s.v. betrachtet das handschriftliche *ašahunara-* als legitime Graphie von /aš-hŭnara-/, doch kann *a* nur einem *ŋh*, nicht aber einem *h* vorgeschlagen werden (vgl. *niš.harətar-* : *niš.aŋharəθrī-*).

2.6. *aš.manah-* etwa 'von großem Mut' Yt. 17.13: *aēšąm uštråŋhō baiieṇte/saēni.kaofa* +*aš.manaŋhō*[10] 'Ihre Kamelhengste ...en (*baiieṇte*?), die spitzhöckrigen[11], sehr mutigen' (nach Lommel; Bartholomae 'mit starker Begierde, Brunst')[12]. Das Beiwort bezieht sich wohl spezifisch auf die Aggressivität der brünftigen Kamelhengste, wie denn auch im folgenden – nach einer leider verdorbenen Verszeile – von ihren Brunftkämpfen die Rede ist (+*pərətəmna vaδairiiauuō* 'wenn sie brünftig kämpfen'). Vgl. auch die eingehende Schilderung Yt. 14.11–13.

2.7. *aš.varəcah-* 'von großem Ansehen', Beiname des Türers Fraŋrasiian und des Kauui Usan: Yt. 19.57f. *fraŋrase tūrō aš.varəcå*; Yt. 5.45 *auruuō* +*aš.varəcå*[13] *kauua usa*; Az. 2 *bauuāhi ... aš.varəcå yaθa kauua usa*. Die Form ist F. 8 (= 398 Klingenschmitt[14]) zu *varəcå* verdorben, aber korrekt als *KBYR wlc'hw* übersetzt.

2.8. *aš.x*^{v}*arənah-* 'großen Ruhmesglanz habend'[15], vom heiligen Wort: *mąθrō spəṇtō yō aš.x*^{v}*arənå* (Yt. 12.2, syntaktisch in der Luft hängend; V. 22.2,6; V. 19.16 als Akk.); *mąθrəm spəṇtəm aš.x*^{v}*arənaŋhəm* (Y. 2.13 u. Par. in Prosa, aber gleichfalls als Achtsilbler formuliert). Vgl. die freie Phrase *masō ... x*^{v}*arənaŋhō* Yt. 5.96.

2.9. +*aš.kāma-* 'große Wünsche habend' V. 20.1 Gl. Der NSg. *aškāmō*, nur in K1 und L4, glossiert *yaoxštiuuatąm*, PÜ *k'mk'wmnd* 'Wünsche habend'. Bartholomae s.v. übersetzt unnötigerweise 'mit vielen Wünschen'. Zu elam.-apers. *Maš-ka-ma* s. Anm. 49.

2.10. *aš.mižda-* 'großen Lohn gewährend' Y. 55.2: *tå* (scil. *gāθå*) *nō buiiąn humiždå aš.miždå ašō.miždå* '(die Gāθās) sollen uns guten Lohn, großen Lohn, den Lohn des Aša gewähren'.

2.11. *aš.pairika-* 'von großen Pairikās (d.i. von Erzhexen) begleitet', Beiname des Pitaona Yt. 19.41: *pitaonəmca aš.pairikəm*. Bartholomae gibt als wörtliche Übersetzung 'der viele Pairikās hat', Lommel hat 'hexenreich'. Diese Auffassung, nach der *aš.pairika-* gleichbedeu-

tend mit einem **pouru.pairika-* wäre, läßt sich zwar nicht strikt widerlegen, setzt aber für *aš.-* eine Uminterpretation von 'groß' zu 'zahlreich' voraus, deren Annahme sonst nirgends notwendig ist. Daß *mazånt-* sonst nicht von daēuuischen Wesen belegt ist, halte ich für Zufall[16].

2.12. *aš.pacina-* 'großes Kochen habend, wo viel gekocht wird' Yt. 5.130: *yaθa azəm huuāfritō/masa xšaθra niuuānāni/aš.pacina stūi.baxəδra*[17]*/fraoθaṯ.aspa* $^{+}x^{v}$*anaṯ.caxra xšuuaēβaiiaṯ.aštra/aš.baouruua niδātō.pitu hubaoiδi* 'daß ich wohlbeglückt große Reiche gewinne, wo viel gekocht und kräftig ausgeteilt wird, wo die Pferde schnauben, die Räder sausen, wo man die Peitsche schwingt(?)[18], wo es viel Nahrung (s. gleich) gibt, wo Speisen hingestellt sind, wo es gut duftet'. Falls die KE von *aš.pacina* und *stūi.baxəδra* konkrete Bedeutung hatten, könnte man die beiden Komposita etwa mit 'wo es große Kochtöpfe gibt' (vgl. rigved. *pácana-* 'Kochgerät' 1.162.6), 'wo es kräftige Portionen gibt' übersetzen.

Bei der Interpretation von *masa xšaθra* als APl. mit der Bed. 'große Herrschaftsgebiete, Reiche' folge ich Bartholomae u.a. Dagegen faßt Kellens, Les noms-racines de l'Avesta (1974) 77, 356 *masa xšaθra* als ISg. auf ('afin que je . . . sois vainqueur grâce à un grand pouvoir'), übersetzt aber die Epitheta nicht. Da man annehmen kann, daß av. *mas-* wie *maz-* (= ved. *máh-*) flektiert hat, stellt *masa* als NAPl. (: rigved. *mahā́*[*ni*] < idg. **meǵōh*$_2$) entgegen Kellens kein Problem dar (s. noch Anm. 19).

3. Bei zwei Komposita, *aš.baouruua-* und *aš.vaṇdra-*, deren KE als Simplicia nicht belegt sind, läßt sich die Klassenzugehörigkeit nicht ganz sicher bestimmen.

3.1. *aš.baouruua-* 'viel Nahrung habend' Yt. 5.130 (s.o. 2.12) und ähnlich Yt. 17.7: *tē narō xšaθra xšaiieṇte/aš.baouruua niδātō.pitu hubaoiδi/yahmiia* . . . 'Diese Herren gebieten über Reiche (ein Reich?)[19], wo es viel Nahrung gibt usw., wo (in welchem?) . . .'. Das KE von *aš.baouruua-* gehört zu indoiran. **bharu̯a-* 'kauen, verzehren' (rigved. *bhárvati*). Bartholomae (implizit Wb. 946 u.) und Duchesne-Guillemin 131 sehen in der Bildung ein adjektivisches DK zu einem Simplex **baouruua-* 'kauend', das Kompositum bedeute ('wo man viel kaut' d.i.) 'wo es viel zu kauen, zu essen, wo es reichlich (feste) Nahrung gibt' (Wb. 263). Ich postuliere mit Justi, Handbuch der Zendsprache (1864) 44, 208 schon deshalb ein PK, weil vergleichbare adj. DK[20] bzw. synthet. NAg. mit *aš.-* sonst nicht nachzuweisen sind. Dazu kommt das Problem einer Verbindung von *xšaθra-* mit Nomen agentis (vgl. Anm. 18).

Gleichfalls als PK kann der altpers. PN *Gaubaruva-*[21] aufgefaßt werden ('Rind[fleisch] als Nahrung habend', Typus ved. *ukṣā́nna-* 'Stiere als Speise habend')[22]. Als genauere Bedeutung des erschlossenen iran. **baru̯a-* mag man 'trockene Nahrung' (Gegensatz vl. av. *pitu-* 'gekochte Speise') vermuten. Zu mitteliran. Verwandten s. Bailey, Dictionary of Khotan Saka (1979) s.v. *bāṃrai.*

Mit av. *aš.baouruua-* ist weiters rigved. *sū́bharva-* (zweimal von Rindern) zu vergleichen, für das semantisch ein PK 'gute Nahrung habend, wohlgenährt' genauso gut paßt wie 'gut kauend, fressend': RV 10.102.5 *sū́bharvaṁ* . . . *sahásram/gávām* 'ein wohlgenährtes Tausend Kühe' (Geldner); 10.94.3 (wo die Preßsteine mit Stieren verglichen werden): *vṛ̥kṣásya śā́khām aruṇásya bápsatas/té sū́bharvā vṛ̥ṣabhā́ḥ prém arāviṣuḥ* 'Indem sie nach dem Zweige des rötlichen Baumes schnappen, haben die gutkauenden Stiere (dabei) losgebrüllt' (Ge.). Statt 'gut kauend' kann auch hier 'gute Nahrung erhaltend' oder 'wohlgenährt' übersetzt werden (vgl. auch 11d, wo die Preßsteine *supīvás-* 'feist' genannt werden). Falls wir es, wie

ich glaube, auch bei *sū̆bharva-* mit einem PK zu tun haben, kann seine Verwendung darauf hinweisen, daß **bharu̯a-* wenigstens im Ind. 'Viehfutter', speziell vl. 'Trockenfutter' bedeutete. Gerade in diesem Fall läßt sich das irreguläre *ū* in *sū̆bharva-* leicht erklären, nämlich als Übertragung aus *sūu̯ávasa-* 'gute Weide habend' (*u̯ávasa-* war dann gewissermaßen das Gegenteil von **bharu̯a-*). Problematisch ist natürlich bei *sū̆bharva-* die Betonung des KA, die sonst bei PK mit *su-* sehr selten[23], bei adj. DK indessen häufig ist. Vielleicht hat sich der Akzent von *sū̆bharva-* nach einer semantisch verwandten Bildung gerichtet (vgl. z.B. *sv-àśita-* 'wohlgespeist' RV 10.28.1), was besonders bei etwaiger ursprünglicher Paroxytonese des PK verständlich wäre: **su̯-bhárua-* konnte allzuleicht als 'gut zu verzehren' mißverstanden werden.

Ein weiterer Beleg von *aš.baouruua-* liegt m.E. Yt. 17.8 vor, wo es gleich nach der oben zitierten Stelle 17.7 heißt: *aēšąm nmānå huuiδātå/gaosūrąŋhō hištənte/aš.paouruuå darəγō. upastəe* 'Deren wohlgebaute Häuser stehen da rinderreich, *aš.paouruuå*, zu langer Unterkunft'. Im Gegensatz zu Bartholomae (gefolgt von Duchesne-Guillemin 131), der das handschriftliche *aš.paouruuå* halten will ('die weitaus ersten'), scheint mir B. Geigers Konjektur zu *+aš.baouruuå* (Amәša Spәntas, 1916, 112[1]), die auch Lommel annimmt, unumgänglich zu sein. Erstens kann es schwerlich auf Zufall beruhen, daß *aš.b°* und *aš.p°* innerhalb weniger Zeilen in ganz ähnlichem Kontext aufeinanderfolgen (daß *baouruuå* einmal bewahrt, das zweitemal aber durch das geläufige *paouruuå* ersetzt wurde, stellt bei der Überlieferungslage von Yt. 17 kein Problem dar); zweitens findet *aš.paouruua-* entgegen Duchesne-Guillemin a.O. in *hupauruua-* Y. 52.3 keine Parallele: *vaŋᵛhīšca aδå vaŋᵛhīšca aṣaiiō/ hupauruuå vahehīš aparå* 'die guten Darbringungen und die guten Belohnungen, die guten früheren, die (noch) besseren künftigen'. Während *hupauruua-* im KA funktionell ein Adjektiv enthält ('der gute frühere, der früher gut war'[24]), könnte *aš.-* natürlich nur die Bedeutung eines Adverbs haben[25].

3.2. *aš.vaṇdra-* 'großen Lobpreis habend, sehr gepriesen' Yt. 19.9 u. Par.: *uγrəm kauuaēm xᵛarənō/mazdaδātəm yazamaide/aš.vaṇdrəm uparō.kairīm* 'Den mächtigen kauuischen Ruhmesglanz, den mazdägeschaffenen, verehren wir, den sehr gepriesenen, überlegen wirkenden'. Bartholomae setzt *+aš.vaṇdara-* an (PK mit einem **vaṇdara-* m.), doch ist erstens *aš.vaṇdra-* besser bezeugt[26] (dazu kommt noch das aus unserem Wort entstellte *vaṇdrəm* F. 8 = 397 Kli.), und zweitens steht vor *uparō.kairiia-* immer ein dreisilbiges Vorderstück, vgl. Y. 9.10 *āaṯ aniiō uparō.kairiiō*, Yt. 8.4 *bərəzaṇtəm uparō.kairīm*, Yt. 15.12 *vaiiuš yō uparō.kairiiō*, das Wort ist demnach fünfsilbig[27]. Das vorauszusetzende **vaṇdra-* 'Lobpreis' von *vaṇd-* 'preisen' ist m.E. aus einem Adj. **vaṇdra-* 'preisend'[28] substantiviert, vgl. rigved. *vandā́ru-* 'Lobpreis' (4.43.1, 6.4.2), das durch Ellipse aus *váco vandā́ru* 'preisendes Wort' (5.1.12) von *vandā́ru-* 'preisend' (m. 1.147.2) hervorgegangen ist.

Als Alternative zur hier gegebenen Interpretation von *aš.vaṇdra-* als PK käme die als adj. DK 'sehr gepriesen' zu einem **vaṇdra-* 'gepriesen' in Betracht (so Duchesne-Guillemin 131)[29].

4. Fürs Gav. haben Insler und Kellens drei PK mit *aš.-* durch Konjekturen herzustellen versucht.

4.1. Einen gav. Beleg von *aš.aojah-* (s.o. 2.4) finden sowohl Insler (Gāthās 224) als auch Kellens (StIr 4, 152ff.) in *+aš.aojå nāidiiåŋhəm* Y. 34.8 'der sehr mächtige[30] den schwächeren', wo bisher *as aojå* gelesen wurde. Zur reichlich unklaren Gesamtinterpretation der

Strophe verweise ich auf die Diskussion der beiden Gelehrten. Bei *as* müßte es sich um einen relativ alten Überlieferungsfehler handeln, da dieses der PÜ (*ʿYTʾ* 'ist') zugrunde liegt und auch handschriftlich weitaus besser bezeugt ist als *aš* (nur *aš.aojiiå* S1 und *aṣaojiiå* in Manuskripten des Indischen Vidēvdād Sāda und des Yasna Sāda). Wahrscheinlich aus unserer Stelle, aber interessanterweise ohne *aš*, ist Y. 57.10 *yaθa aojå*[31] *nāidiiānhəm* 'wie der mächtige den schwächeren' übernommen.

4.2. *+aš.xratu-* 'von großer Geisteskraft' Y. 31.9.: *θβə̄ ā gəuš tašā +aš.xratuš/mainiiuš* 'Thine was the fashioner of the cow, the spirit of great determination' (Insler, Gāthās 184). Zur gleichen Auffassung kommt wiederum Kellens StIr 4.155. Auch hier wurde bisher mit der PÜ (*ʿYTʾ*) *as* gelesen[32].

Zu jav. *aš.xraθβastəma-* s.u. 5.2. Vgl. noch die freien Phrasen *xratūm . . . masitəm mazånṭəm* Y. 62.4 und *masiiå . . . xratuš* Yt. 10.107.

4.3. *+aš.īšti-* 'große Macht habend' Y. 44.9: *θβāuuąs +aš.īštiš mazdā* 'someone of great power like Thee, wise lord' (Insler, Lg 47, 1971, 575[7] und Gāthās 69, 246; zustimmend Kellens, StIr 4.152[14]). Bisher las man *asīštiš* (nach Humbach 'Anweiser'), was wohl zumindest schon die Lesart der Yasna-Stammhandschrift war (*aš°* nur *ašīstīš* S1 J4 und *ašīstiš* J6 Jm1; S2). Die PÜ *tyc* 'schnell' beruht auf Verwechslung mit *asīšta-* 'der schnellste' Y. 30.10, 34.4 (an beiden Stellen *aš°* als v.l.).

5. Die mit *aš.-* gebildeten Superlative erfordern eine eigene Behandlung. In einem oder mehreren Superlativen zu PK mit *aš.-* scheint dieses nämlich zu einem Augmentativpräfix in der Bedeutung des deutschen 'aller-' uminterpretiert worden zu sein. In Neubildungen konnte dann *aš.-* einfach einem schon existierenden Superlativ vorgeschlagen werden. Im einzelnen bleiben manche Unsicherheiten bestehen. – Der einzige Komparativ wird hier mitbehandelt.

Bartholomae s. vv. legt den Superlativen jeweils bereits mit *aš.-* gebildete Positive zugrunde. Duchesne-Guillemin, der Comp. 131 die Superlative kommentarlos unter den adjektivischen DK einordnet, müßte wohl von Syntagmen à la homer. *μέγ' ἄριστος* ausgehen.

5.1. *aš.aojastara-* 'noch mächtiger' V. 9.48: *aēša druxš yā nasuš aš.aojastara varəδaiiete yaθa para ahmāṯ as* 'Diese Druj Nasu wächst zu einer noch mächtigeren heran als sie zuvor (schon) war'. Die Form ist regulärer Komparativ zum PK *aš.aojah-*. Ob auch im Komparativ *aš.-* zu einem Präfix uminterpretiert wurde (etwa im Sinne von deutsch 'noch'), läßt sich mangels weiterer Beispiele nicht entscheiden.

Der zugehörige Superlativ ist *aš.aojastəma-* 'der allermächtigste' Y. 9.8 u. Par. (Text oben 2.4.). Wegen der Existenz des Adjektivs *aojah-* und der zumindest potentiellen des zugehörigen Superlativs **aojastəma-* (vgl. ved. *tavás-, tavástama-*) scheint es nicht unwahrscheinlich, daß die Neuauffassung von *aš.-* als verstärkendes Präfix gerade in *aš.aojastəma-* erfolgte. Dabei konnte man *aš.aojastəma-* als verstärktes *aojastəma-*, aber auch als eine direkt auf *aojah-* beruhende Bildung interpretieren.

Auf neue Weise gebildet ist das gleichbedeutende *aš.aojišta-* im späten Fr.W. 8.2: *hā druxš +aš.aojišta aŋhaṯ* 'Diese Druj wird die allermächtigste sein'. Hier wurde *aš.-* dem Superlativ *aojišta-* 'der mächtigste' präfigiert.

5.2. *aš.xraθβastəma-* 'der allerweiseste' Yt. 10.141: *aojištaṇąm asti +aojištō/taṇcištaṇąm asti +taṇcištō*[33] */baγaṇąm asti aš.xraθβastəmō* 'Unter den sehr starken ist er der stärkste,

unter den sehr kühnen ist er der kühnste, unter den Göttern ist er der allerweiseste'. Als regulärer Superlativ von **aš.xratu-* (vgl. 4.2.) wäre **aš.xratutəma-*, vom Simplex *xratumat-* **xratumastəma-* zu erwarten. Belegt sind stattdessen *aš.xraθβastəma-* und *xraθβišta-* (Yt. 13.80 u. Par., Y. 1.1). Die beiden Formen sind zweifellos direkt dem Paar *aš.aojastəma- : aojišta-* nachgebildet. Einen Positiv **aš.xraθβaṇt-* (Bartholomae, Wb. 260, 537) hat es nie gegeben.

5.3. *+aš.vərəθrająstəma-* 'der allersiegreichste' Y. 9.15: *yō aojištō yō taṇcištō/yō θβaxšištō yō āsištō/yō +aš.vərəθrająstəmō*[34] */abauuaṯ +maniiuuå +dāmą* '(Zaraθuštra,) der der stärkste, der der kühnste, der der eifrigste, der der schnellste, der der allersiegreichste der Schöpfung der beiden Geister wurde'. Die gleiche Folge, nur mit litaneihafter Auflösung, Yt. 10.98 = 135: *yō aojištō yazataṇąm/yō taṇcištō yazataṇąm/yō θβaxšištō yazataṇąm/yō āsištō yazataṇąm/yō +aš.vərəθrająstəmō*[35] *yazataṇąm/fraxštaite paiti āiia zəmā* '(Miθra,) der . . . als allersiegreichster unter den Göttern auf dieser Erde vordringt'. Zur philologischen Begründung der Konjektur *+aš.°* s. Seiler, Relativsatz, Attribut und Apposition (1960) 95f., Humbach, FS Lentz (1974) 91, Kellens, StIr 4.151f. Die Alternative, daß *as* zu halten ist und aus einer Stelle stammt, an der die Kopula berechtigt war (so Bartholomae, Wb. 279, Fn. 36), ist weniger wahrscheinlich. Vgl. noch Gershevitch, Hymn to Mithra 248f. In *+aš.vərəθrająstəma-*, wenn richtig hergestellt, liegt der durch *aš.-* verstärkte Superlativ *vərəθrająstəma-* 'der siegreichste' (von *vərəθrajan-*) vor.

5.4. *ašəθβō.zgatəma-* 'der allerregsamste(?)' Y. 13.2: *ašəθβō.zgatəmą gauuāstriiāuuarštəmą naraṃ ašaonąm vāstriiehē fšuiiaṇtō ratūm āmruiiē* 'Die allereifrigsten, die landwirtschaftlich tätigsten unter den ašagläubigen Männern rufe ich auf als Ratav des viehzüchtenden Ackerbauers' (Wolff). Die Bildung gehört wohl zum Hapax *θβązjaiti* Yt. 19.58, nach Gershevitch, TPhS 1964, 16f. 'is aroused, startled, upset'. Über die Derivationsgeschichte läßt sich nichts mehr ermitteln.

5.5. Im selben Stück steht das völlig unklare *+aš.xrā.xᵛanutəma-*. Y. 13.3 (pseudogav.)≈ Vr. 3.5 (normaljav.): *amə̄šascā spəṇtą saošiiaṇtascā dąhištą +arš.vacastəmą aiβiiāmatəmą +aš.xrā.xᵛanutəmą*[36] 'die Amə̄ša Spəṇtas und die Saošiiaṇts, die kundigsten, die am richtigsten formulieren, die übermächtigsten, die . . .sten'. Nach Bartholomae s.v. ist die Form Superlativ von einem PK **aš.xrāxᵛanu-*. Das zugrunde liegende **xrāxᵛanu-* m. gehöre zum Verbum *xrāŋhaiia-* 'erschüttern' und sei eine Bildung wie ved. *vagvanú-* 'Getön'. Als Bedeutung setzt Bartholomae 'der am allermeisten antreibende, anregende' an.

Das genannte Verbum wäre nur Yt. 10.36 belegt: *frā maiδiiānəm xrāŋhaiiete/spāδahe xruuīšiiaṇtahe* 'Erschüttert wird die Mitte des blutdürstigen Heeres' (Wolff), 'is quaking' (Gershevitch). Humbach, FS Lentz 87[7] vermutet, *xrāŋhaiia-* "could belong to the root *karš* 'to plough' just as *θrāŋhaiia-* to the root *tarš-* 'to tremble'" (was unter anderem Schwebeablaut voraussetzte). Er übersetzt 'he cuts asunder the center of the blood-thirsty army'. Indessen ist, wie auch Kellens, Verbe 148[44], 262, gesehen hat, *+θrāŋhaiiete* zu lesen (*θarāŋhiieti* H4, das einen von der F1-Linie unabhängigen Text bietet) und zu übersetzen 'In Schrecken versetzt er usw.'.

Dazu kommt, daß das von Bartholomae als morphologische Parallele angeführte ved. *vagvanú-* (RV 9.3.5 *āvíṣ kṛṇoti vagvanúm* 'er bringt Getön hervor') selber isoliert ist und dem Verdacht unterliegt, eine aus *vagnú-* ds. unter dem Einfluß von *vagvaná-* 'geschwätzig' und einer der Formationen auf *-anú-* (vgl. 8.21.14 *yadā́ kṛṇóṣi nadanúm* 'wenn du das Schlachtgeschrei erhebst' [Ge.]) umgestaltete Augenblicksbildung zu sein, s. Renou, ÉVP 8.51.

Die PÜ von *+aš.xrā.xᵛanutəma-, KBYR hlt'-krt'ltwm* 'am meisten Verstand verschaffend' beweist, daß die überlieferte Wortform so schon dem Übersetzer vorgelegen hat: *krt'ltwm* will das an *kwn-* 'machen' anklingende *xᵛanutəmą* wiedergeben. An eine (auch in den Vr. verschleppte) Korruptel – etwa aus **aš.xraϑβastəmą* – darf daher kaum gedacht werden.

5.6. *aš.xᵛarətəma-* 'der allergefräßigste' V. 3.20 = 9.49: *aš.xᵛarətəmaēibiiō spəṇtō.maniiauuaṇąm dāmaṇąm +kərəfš.xᵛarąm kərəfš* (statt *kəhrpəm*) *paiti.nisrinuiiāṯ vaiiąm kahrkāsąm* 'Den allergefräßigsten unter den fleischfressenden Geschöpfen des heiligen Geistes überantworte man die Leiche, den Geiervögeln'. Am einfachsten wäre es, in *aš.xᵛarətəma-* den Superlativ zu einem synthet. NAg. **aš.xᵛar-* 'sehr fressend' zu sehen. Da jedoch genau vergleichbare Bildungen fehlen (zu *aš.bərət-* s. gleich), muß erwogen werden, ob die Form nicht vielmehr als mit *aš.-* verstärktes **xᵛairišta-* 'am meisten fressend' intendiert ist, wobei *-išta-* durch das in Komposita normale *-təma-* (es würde z.B. **kərəfš.xᵛarətəma-* heißen) ersetzt worden wäre.

6. Synthetische Komposita. Wir finden (von dem soeben behandelten *aš.xᵛarətəma-* abgesehen) nur noch ein Nomen agentis und drei eng zusammengehörige Nomina actionis.

6.1. *+aš.bərət-* '?' Yt. 13.23: *yå +aš.bərətō*[37] *yå uγrārətō/yå huuārətō yå vazārətō/yå taxmārətō yå zaoiiārətō/yå zaoiiå vaŋhuϑβaēšu* '(les frauuaṣis,) qui apportent beaucoup, qui s'élancent puissantes, qui s'élancent d'elles-mêmes, qui s'élancent avec puissance, qui s'élancent hardies, qui s'élancent dignes d'être invoquées, qu'on doit invoquer dans les dangers' (Kellens, Noms-racines 128).

Die traditionelle Übersetzung 'reichlich bringend' ist keineswegs sicher. Auf eine andere Möglichkeit führt die Interpretation der übrigen Komposita unserer Stelle, die bei *zaoiiārət-* einzusetzen hat. Falls *zaoiiārət-* nicht ein sonst unbelegtes **zaoiiă-* 'Anrufung' enthält ('auf den Ruf sich aufmachend' Bartholomae s.v.), sondern, wie wahrscheinlich, das auch gleich im folgenden dreimal erscheinende Gerundiv *zaoiia-*, kann dieses funktionell nicht Adverb sein: 'die sich auf anzurufende Weise in Bewegung setzen' wäre nicht sinnvoll. Vielmehr muß *zaoiia-* in diesem Fall die Funktion eines Prädikatsadjektivs haben 'die sich als anzurufende in Bewegung setzen'[38]. Es wäre dann weiter wahrscheinlich – und Kellens übersetzt entsprechend –, daß die parallelen *uγrārət-* und *taxmārət-*[39] als 'die sich als starke, kühne in Bewegung setzen' zu interpretieren sind ('kräftig, kühn sich aufmachend' Bartholomae s.vv., vgl. Duchesne-Guillemin 80); *uγra-* und *taxma-* sind ja geläufige Epitheta der Frauuaṣis. Zur vorauszusetzenden Konstruktion **yå uγrå* (etc.) *iiarəṇti* vgl. z.B. Yt. 5.3 *yå amauuaiti fratacaiti* 'die als mächtige vorwärtsfließt'[40]. Damit eröffnet sich die Möglichkeit, auch im KA von *aš.bərət-* funktionell das Adjektiv 'groß' zu sehen und vorläufig zu übersetzen 'die als große bringen'. Man vergleiche dazu besonders Yt. 13.64: *yå masiiehīš ahmāṯ/yå aojiiehīš ahmāṯ/yå tąšiiehīš ahmāṯ/. . ./yaϑa vaca +framruuāire* '(die Frauuaṣis,) die größer, stärker, kühner sind ... als sie durch das Wort beschrieben werden können'. Hier geht den Komparativen von *uγra-* und *taxma-* der von *mas-* = *mazåṇt-* voraus, geradeso wie an unserer Stelle *aš.-*, die Kompositionsform von *mazåṇt-*, vor *uγra-* und weiterhin *taxma-* steht.

Die Interpretation des KE *-bərət-* als 'bringend' ist ebenfalls nicht unproblematisch. Außer in der Bedeutung 'reiten' (die Frauuaṣis sind aber nicht beritten) kommt verbales *bar-* nie ohne direktes Objekt vor, und auch die Vorderglieder sämtlicher jav. synthet. NAg. auf *-bərət-, -bara-, -barana-* fungieren als Objekte von *bar-*[41]. Außerdem ist Unterstützung das

einzige, was die Frauuaŝis bringen (*barən upastąm* Yt. 13.1 = 19). Nicht ganz mit Unrecht sagt Herzfeld, Zoroaster and his world (1947) 499 "'much-bringing' is nothing"[42]. Auch der asymmetrische Bau der Strophe – ein Kompositum mit *-bərət-* gefolgt von fünf mit *-ərət-* – ist auffällig. Vgl. dagegen z.B. die mit *yō fraxšti.då yō āzuiti.då* beginnende Reihe von acht Komposita mit *°då* Yt. 10.65. Nimmt man dazu, daß gerade *aš.-* dasjenige Vorderglied der Serie ist, nach dem *-ərət-* nicht durch Kompositionssandhi verändert worden wäre, muß mit der Möglichkeit gerechnet werden, daß die Form des Textverfassers **až-ərətō* 'die sich als große in Bewegung setzen' lautete.

6.2. *aš.yešti-* 'große Verehrung' Y. 68.9: *āca nō jamiiå auuaŋhe/aš.yeštica huiieštica/ hufrabərətica zaoθranąm* 'Und mögest du uns zu Hilfe kommen aufgrund (unserer) großen Verehrung und guten Verehrung und der guten Darbringung der Opferspenden'.

aš.frāiiašti- 'große Verehrung' und *aš.frabərəiti-* 'große Darbringung' erscheinen in dem deutlich aus dem eben zitierten Stück umformulierten und mit mehr Parallelismus versehenen Passus Yt. 10.77: *āca nō jamiiāṯ auuaŋhe/aš.frāiiaštica zaoθranąm hufrāiiaštica/ +aš.frabərətica zaoθranąm +hufrabərətica.* Sie haben kaum sprachgeschichtlichen Eigenwert.

Wahrscheinlich ist das isolierte *aš.yešti-* zu *huiiešti-*, das einem ererbten und produktiven Typ angehört, nach dem Vorbild der PK (vgl. *humižđå aš.mižđå* Y. 55.2) aufgrund der möglichen Auffassung der komponierten *ti*-Abstrakta als DK hinzugebildet worden. Seiner Bedeutung nach ist *huiiešti-* die komponierte Version von *vaŋhu- yasna-*, vgl. *vaŋhūš yasnąsca* (*vahmąsca*) Y. 23.3 u. Par., ebenso wie *aš.yešti-* die von *mazåṇt- yasna-*, vgl. *mazīštəm yasnəm* Yt. 1.24.

7. Unsere Materialuntersuchung führt zu folgendem Ergebnis: *aš.-* ist Kompositionsform von *mazåṇt-* 'groß', hauptsächlich in PK (2–4). In Superlativen von PK wie *aš.aojastəma-* wurde *aš.-* bis zu einem gewissen Grad zu einem verstärkenden Präfix (deutsch 'aller-') uminterpretiert (5). Eigentliche adjektivische DK, in denen für *aš.-* die Bedeutung eines Adverbs 'sehr' zu erwarten wäre, lassen sich nicht nachweisen. Die dafür angeführten Beispiele wie *aš.vaṇdra-* (3.2.) erlauben jeweils andere Interpretationen. Adverbielle Bedeutung wäre im Normalfall auch bei synthetischen NAg. zu erwarten (vgl. RV 8.46.18 *mahi-ṣváṇi-* 'sehr brausend'[43]). Von den beiden einschlägigen Bildungen ist nun die eine, *aš.xᵛarətəma-*, als Superlativ nicht voll beweiskräftig (5.6.), und die andere, *aš.bərət-*, dürfte funktionell ein Prädikatsadjektiv enthalten (6.1.). Auch die Abstrakta wie *aš.yešti-* 'große Verehrung', die analogisch zu solchen mit *hu-* geschaffen wurden, können adjektivisch aufgefaßtes *aš.-* enthalten (6.2.).

Im Indoiranischen ist der Gebrauch der Kompositionsform eines Adjektivs[44] auch in der Funktion des zugehörigen Adverbs in synthet. NAg. regelrecht (für die anderen einschlägigen Kompositionsklassen gibt das Material zu wenig her)[45]. Das gilt auch für solche Fälle, in denen eine größere formale Differenz zwischen Adjektiv und KA besteht. Vgl. für Caland-Adjektiva rigved. *tuvi-* in synthet. NAg. wie *tuvi-bādhá-* 'stark bedrängend' (: Adj. *tavás-*), jav. *bərəzii-aogəṯ* 'mit hoher (lauter) Stimme sprechend' (: Adj. *bərəz-, bərəzat-*, rigved. Adverb *bṛhát* 'hoch, laut'), *xruui-γnī-* Name einer Daēuuī, wenn eigentlich 'grausam tötend' (: Adj. *xrūra-*). Freilich ist es bei der spärlichen Verwendung von Adjektivadverbien im Indoiran. oft nicht sicher, ob das KA der Funktion nach ein Adverb oder ein Prädikatsadjektiv repräsentiert (letzteres offenbar in *xšuuiβi.vāza-* Yt. 8.37: *xšuuiβi.vāzəm/yō*

auuauuaṯ xšuuaēβō vazaite 'den schnellfahrenden, der so schnell fährt'). Aus historischer Sicht ist die Regel jedenfalls sinnvoll, da ja die synthet. NAg. mit verbalem KE als idg. Typus letztlich mit großer Wahrscheinlichkeit auf uminterpretierte PK mit NAct. als KE zurückgehen.

Daß in unserem Fall weder das KA *aš.-* in eindeutig adverbieller Funktion noch (im Jav.) ein zu *mazå̊nt-* gehöriges Adverb belegt sind, könnte nun ganz einfach Zufall sein: Im RV kommt zwar das Adverb *máhi* öfters vor, aber außer dem schon genannten *mahi-ṣváṇi-* gibt es nur noch ein synthet. Adj., *mahi-vr̥̊dh-* 'großgewachsen' (7.31.10; vgl. *párvataṣ cin máhi vr̥̊ddhó* 'der hochgewachsene Berg' 5.60.3).

Es besteht aber auch die Möglichkeit, daß es im Jav. ein Adverb 'sehr, valde' gar nicht gegeben hat. Auch *vaŋhu-* 'gut' bildet bekanntlich kein Adverb, 'bene' läßt sich nur durch Umschreibung mit figura etymologica ausdrücken (Typus *hubərətəm bar-* 'jemanden gut halten, pflegen'). In diesem Fall wäre auch das Fehlen von Komposita, die ein Adverb 'sehr' der Funktion nach enthalten, verständlich[46]. Man beachte noch die figura etymologica (statt eines Adverbs) für 'maxime' in *kō paoirīm imąm ząm mazišta xšnaoma xšnāuuaiieiti* (V. 3.12) 'Wer befriedigt erstens diese Erde mit größter Befriedigung (d.h. am meisten)?' (ein zweites Beispiel V. 18.61).

8. Wir wenden uns nun kurz der morphologischen Bestimmung von *aš.-* zu. Bartholomaes Interpretation, nach der *aš.-* eine Bildung mit 'adverbiellem' **-s* wäre[47], ist nicht haltbar. Während Nominalstämme, die als Kompositionsformen von Adjektiven verwendet werden, wie oben ausgeführt, auch adverbielle Funktion haben können, trifft das umgekehrte nicht zu: Adverbia sind nur dann als KA von PK zulässig, wenn sie prädikativ, d.h. in Verbalphrasen mit der Kopula verwendet werden können[48], was gerade bei Adjektivadverbien nicht der Fall ist (vgl. Hoffmann, Aufsätze 2.339ff.). Nicht mit der Kopula verbindbare Adverbia erscheinen, ihrer syntaktischen Rolle gemäß, in synthet. NAg., NAct. und passiven Verbaladjektiven, sowie allenfalls in adjektivischen DK. Sie können natürlich auch in solchen formal wie PK aussehenden Bildungen auftreten, deren KE sekundär wie die von synthet. Komposita verwendbar geworden waren[49]. Doch übernehmen sie nicht die Rolle der ihnen allenfalls entsprechenden Adjektiva in eigentlichen PK.

Wenn somit ein Adverb nicht in Frage kommt, kann **m̥̊ĝs-*, die Vorform von *aš.-*, nur die schwundstufige Stammform eines *s*-Stamms **meĝ-es-* darstellen. Die Reduktion, zu der man formal **mn̥̊s-dhéh$_1$-* von **ménes-* in av. *maz-dā-* vergleiche, ist regelrecht, da PK im Idg. zur Zeit der Schwundstufenbildung auf dem KE betont waren. Der Stamm **meĝes-* kann nun auf zweierlei Weise interpretiert werden: einmal als neutrales Abstraktum 'Größe', belegt in ved. *máhas-*[50] und av. *mazah-*; zum zweiten als ein daraus durch interne Derivation abgeleitetes Possessivadjektiv **meĝés-* 'Größe habend = groß', vielleicht belegt in ved. *mahá̤ḥ*, das an einigen Stellen am einfachsten als neutrales Adjektiv aufgefaßt wird (z.B. RV 1.3.12 *mahó árṇaḥ* 'große Flut'). Possessivadjektiva beliebiger Bildungsweise konnten aber, wie ich an anderer Stelle zu zeigen versuchen werde, im Idg. formal nicht als KA fungieren[51]. Diese Regel und der ohnehin zweifelhafte Status von **meĝés-* machen es wahrscheinlich, daß **m̥̊ĝs-* zum Abstraktum **méĝes-* 'Größe' gehört. Ein idg. PK der Form **m̥̊ĝs-X* ist als 'ein X habend, das mit Größe versehen (= groß) ist' aufzulösen. Der Kompositionstyp ist der von ved. *íṣu-hasta-* 'Pfeile in der Hand habend', ursprünglich 'eine Hand habend, die mit Pfeilen versehen ist'. Hier könnte auch idg. **dus-* 'schlecht' einzuordnen

sein, dem vielleicht ein Substantiv **déu̯es-* 'Mangel' zugrunde liegt[52]: **dus-X* 'ein X habend, das mit Mangel versehen (= mangelhaft) ist'.

Im gav. Adverb *maš* < **mégˆ-s* möchte ich aus formalen Gründen am ehesten eine archaische Nom.-Akk.-Form von **méges-* 'Größe' sehen (vgl. **mén-s* in av. *mąz-dā-*), doch bin ich mir über die syntaktische Berechtigung einer solchen Interpretation nicht im klaren (vgl. Brugmann, Grundriß 2.2.688). Als Adverb weiter verbreitet ist idg. **mégˆh₂* in ved. *máhi*, gr. *μέγα* und wenigstens indirekt aisl. *miǫk*.

Anmerkungen

[1] Abzulehnen Darmesteter, MSL 2 (1875) 307 und Collitz, JAOS 51 (1931) 160ff., die *aš.-* mit gr. *ἐξ* usw. gleichsetzen.

[2] Humbach, Gathas 2.45f. stellt *maš* vermutungsweise zu jav. *mošu* 'alsbald', lat. *mox*.

[3] Wenn im folgenden von *mazånt-* gesprochen wird, sind *maz-* und *mas-* stets miteinbegriffen.

[4] *aš.dānuṇąmca, kasu.dānuṇąmca* ohne Var.

[5] Zur Syntax s. Reichelt, Elementarbuch 258.

[6] So nach Bartholomae u.a.; Lommel übersetzt, sachlich unangemessen, 'zu den Getreidefeldern, die reichlich Körner bringen und zu den Weiden, die wenig Körner bringen'.

[7] Herzustellen wohl auch H. 2.9 statt des überlieferten *amaiiå huraoδaiiå*, doch lag *amaiiå* schon dem Pahlavi-Übersetzer vor, der das Wort mit *'m'wnd* wiedergibt.

[8] *ašahunarəm* F1 Pt1 L18; *ašahe.narəm* P13.

[9] Der Passus ist verwandt mit Yt. 5.91 *haδa.hunarō tanu.mąθrō* und Y. 57.33 *taxmahe tanu.mąθrahe /. . ./ bāzuš.aojaŋhō raθaēštå.*

[10] *aš.manaŋha* ohne Var. Es empfiehlt sich nicht, die überlieferte Form zu halten und als thematisiert aufzufassen, da Thematisierungen von *s*-stämmigen PK praktisch nicht vorkommen. Deskriptiv am besten bezeugt, wenngleich nur an minderwertigen Stellen, ist *pouru.xᵛarənaŋha-* 'viel Ruhmesglanz habend': *pouru.xᵛarənaŋhō aṣ̌auua zaraθuštrō* V. 19.3, *pouru.xᵛarənaŋha ahura mazda* (Vok.) Vyt. 24, *yazata pouru.xᵛarənaŋha* (Pl.) Ny. 3.11 u. 5.6 (auch Perseveration möglich), *xᵛarənō . . . pouru. xᵛarənaŋhəm* (Akk.) Yt. 18.1 (einfach die m. Form, wie das danebenstehende *huš.hąm.bərətəm* statt *ˈbərəṯ* nahelegt). – Vgl. weiters V. 6.16, 18, 20, wo der ASg. *astəm* 'Knochen' der Reihe nach mit *ərəzu.stauuaŋhəm, pərəsu.masaŋhəm, ⁺bī.ərəzu.stauuaŋhəm, bipərəsu.masaŋhəm, bāzu.stauuaŋhəm, sraoni.masaŋhəm* verbunden wird, bei denen es sich wohl gleichfalls lediglich um die m. Formen handelt. – Schließlich steht die Form *stəhrpaēsaŋhəm* nach Bartholomae s.v. an drei Stellen bei Neutra. Zwei davon entfallen: Y. 57.21 ist nach Ausweis des Metrums mit Geldner *nmānəm . . . stəhrpaēsəm* zu lesen; Yt. 13.3 (*asmanəm*) . . . *yim mazdå vaste vaŋhanəm /stəhrpaēsaŋhəm mainiiu. tāštəm* heißt nicht, wie man zu übersetzen pflegt, '(den Himmel,) . . . den sich Mazdā als sternengeschmücktes, geistgeschaffenes Kleid anlegt', sondern vielmehr 'den sich Mazdā als Kleid anlegt, den sternengeschmückten, geistgeschaffenen'. Lediglich Y. 9.26 ist die Phrase *stəhrpaēsaŋhəm mainiiu. tāštəm* mechanisch auf das n. *aiβiiāŋhanəm* übertragen. – Falsch beurteilt wurde von Bartholomae Yt. 13.23 u. Par. *frauuaṣ̌aiiō aṣ̌ōiš baēšaza hacimnå zəm.fraθaŋha dānu.drājaŋha huuarə.barəzaŋha*, wo natürlich, wie auch Kellens, Le verbe avestique (1984) 53 gesehen hat, von *hacimnå* abhängige Instrumentale vorliegen.

[11] Man beachte, daß *stuui.kaofa-*, das einzige andere Kompositum mit *kaofa-*, gleichfalls neben einem *aš.*-Kompositum steht (*aš.bāzāuš*, s. 2.2.).

[12] Vgl. Il 487f. . . . *ταῦρον . . . / αἰθωνα μεγάθυμον.*

[13] *aš.varəcō* ohne Var.

[14] Farhang-i ōim. Edition und Kommentar (Diss. Erlangen 1968).

[15] Ein altpers.-med. PN **Aš-farnah-* ist in der elam. Form *Ašparna* bezeugt (dazu das Hypokoristikon *Ašparnuka*), s. Mayrhofer, Onomastica Persepolitana (1973) 130 mit Lit.

[16] Vgl. RV 1.133.1 *drúho dahāmi sám mahîr* 'Ich verbrenne zur Gänze die großen Lügengeister' (nach Geldner).

[17] Zum Nebeneinander von *aš.-* und *stuuī.-* (so die ursprüngliche Schreibung) vgl. oben 2.2. *aš.bāzāuš stuui.kaofō.*

[18] Der Form nach beruht *xšuuaēßaiiaṯ.* wohl auf einem Transitivum **xšuuaēßaiia-*, das Kompositum wäre demnach (mit Bartholomae s.v. und Duchesne-Guillemin 201) synthet. NAg. 'die Peitschen schwingend'. Für die Figur 'Reiche, die die Peitschen schwingen' = 'wo man die Peitschen schwingt' sind mir allerdings keine Parallelen zur Hand. Andererseits wäre der Ansatz eines intransitiven **xšuuaēßaiia-* (so Lommel), der auf die gut passende Bedeutung 'wo die Peitschen schwirren' führte, morphologisch nicht ganz leicht zu rechtfertigen.

[19] *xšā-* wird bekanntlich sonst mit Gen. konstruiert. Hat der Kompilator unseres Stücks irgendwo *xšaϑra xšaiieṇte*, eigentlich 'sie gebieten mit Macht', vorgefunden und mißverstanden?

[20] Kellens' Konjekturen *+aš.uγra-*, *+aš.dərəzra-* und implizit *+aš.viiāxanaiia-*, *+aš.vərəϑraγniia-*, *+aš. baēšaziia-* (Yt. 14.46; ebenso schon Geldner, Drei Yasht aus dem Zendavesta, 1884, 83) bzw. *+aš.vərəϑrauuan-*, *+aš.amauuan-* (Yt. 14.59), die er selber 'sous toute réserve' vorschlägt (StIr 4, 1975, 154[21]), dürfen auf sich beruhen. Statt *aš* ist *ās* 'war' überliefert, und in der Form, in der uns die beiden Stellen vorliegen (vgl. a.O. 149), ist jedenfalls die Kopula auch gemeint. Abzulehnen ist Geldners (3Yt. 116) Erklärung des PN *ašta.auruuaṇt-* als *+aš.tauruuaṇt-*, s. Mayrhofer, Die avestischen Namen (1977) 25f. mit Lit.

[21] Vgl. Mayrhofer, Die altpersischen Namen (1979) 20f. mit Lit.

[22] Das gleiche gilt für den elam.-altpers. PN *Pid(d)abarma*, falls die geistreiche Deutung von Gershevitch, Studia Pagliaro II (1969) 223f. als **bīda-barva-* 'seed-chewer' (zu ved. *bíja-* 'Getreidekorn') zutrifft.

[23] Vgl. *súgabhasti-* RV 9.72.2 und jüngere, AiGr. 2.1.294 angeführte Beispiele. – Ein synthet. NAg. dürfte durch den Akzent ausgeschlossen sein.

[24] Falsch Bartholomae s.v. ('der reichlich frühere' oder 'voranstehende').

[25] Unwahrscheinlich andere Versuche, mit *aš.paouruua-* zurechtzukommen, bei Geldner, 3Yt. 97, 103 ('mit starken Fugen', zu ved. *párvan-*) und Bailey, Zoroastrian Problems (1943) 6[1] (PK mit iran. **paru̯a-* 'abundance', vgl. gr. πλοῦτος).

[26] Yt. 19.9 *aš.vandarəm* F1 usw., *aša.vaṇdarəm* J10 D; Yt. 19.14 *aš.vaṇdrəm* F1 usw., *aš.vandarəm* H3; Yt. 19.45 *aš.vaṇdrəm* F1 usw.

[27] Für *aš.vaṇdra-* z.B. auch Geldner, 3Yt. 11, Duchesne-Guillemin 131.

[28] Kaum in geschichtlichem Zusammenhang mit dem Uṇādis. 2.13 gelehrten ai. *vandra-*, Bed. nach Ujjvaladatta 'pūjaka-'.

[29] Vgl. noch Klingenschmitt *apud* Mayrhofer, KEWA 3.568 Fn., der das KA von jav. *xᵛaṇdrakara-*, etwa 'schmeichelnd' oder 'gefällig', als **hu-u̯andra-* 'wohllöblich' interpretiert. An den Belegstellen V. 13.46, 48 wird der Hund als *xᵛaṇdrakara-* wie der Knecht bzw. die Hure bezeichnet. Die Auflösung des Gesamtkompositums ist mir unklar ('wohlgelobt machend'?).

[30] Kellens a.O. 153 hält die Form seltsamerweise für einen Komparativ 'beaucoup plus puissant', obwohl er *aojå* und nicht das als v.l. (lectio facilior) gut bezeugte *aojiiå* in den Text setzt. Der Komparativ von *aš.aojah-* hätte im Gav. gewiß wie im Jav. *aš.aojastara-* gelautet, s. 5.1.

[31] *aojah-* als Adjektiv ist noch Yt. 10.140 belegt.

[32] *as* J2.3.6.7.1; *aš* nur im Indischen Vīdēvdād Sāda (L1.2.3 S2 Bb1).

[33] Hss. *aojištəm, tancištəm*; vgl. zur Verderbnis Yt. 15.16 *yaṯ bauuāni xᵛarənaŋᵛhastəməm* (F1 J10) *zātanąm*, wo das unabhängige K40 noch *°tamō* hat.

[34] *as* alle, nur J3 P11 *ās; aš* L1; *aša* L3 P1.

[35] Yt. 10.98 *as* ohne Var. 10.135 *as* F1 Pt1 L18 P13 K15; *aš* H4 M12; deest H3.

[36] Geldner *aŋxrâxᵛanutəmą*, s. die Varianten.

[37] *aŋbərətō* Mf3 K13.38 H5; *aša.barətō* F1 usw. *aša.bərətō* J10.

[38] *zaoiia-* ist im Jav. (außer im Wortsalat V. 1.14) nur Beiwort der Frauuaŋis, vgl. noch Yt. 13.27 u. 148.

[39] Die Bedeutung des KA von *vazārət-* ist unbekannt.

[40] Synthet. NAg., die auf Verbalphrasen mit Prädikatsnomina im Nominativ beruhen, sind grundsätzlich bildbar, aber naturgemäß nicht häufig nachzuweisen. Vgl. unten 7 zu *xšuuiβi.vāza-*.

[41] Die Alternative, *aš.bərət-* als 'Großes bringend' aufzufassen, entsprechend einem freien Syntagma **mazāṯ bar-* o.ä., dürfte nicht zum Ziel führen. Es läßt sich nämlich nicht wahrscheinlich machen, daß *aš.-* auch ein (ohnehin unbelegtes) Substantiv 'das Große' mitvertreten konnte: *hu-* und *duš-* haben jedenfalls nie die Funktion der neutralen Substantiva *vohu-* bzw. *aka-*. 'Gutes wirkend' heißt vielmehr *vohu-uarəz-*, 'das Schlechte schaffend' *akō.dā-*.

[42] Herzfeld möchte **aspa.rt-* 'fighting on horseback' oder **asa.brt-* 'riding on horses' herstellen.

[43] Daneben *tuviṣváṇi-* wie av. *stuuī.-* neben *aš.-*, s. Anm. 17.

[44] Als Kompositionsformen von Adjektiven können Adjektiv- und in bestimmten Fällen (einiges bei Vf., FS Risch, 1986, 393ff.) Substantivstämme dienen; zu letzteren rechne ich mit Schulze *apud* Fraenkel, KZ 42 (1909) 124[2] auch die KA mit Caland-*i*, worüber anderswo.

[45] Vgl. AiGr. 2.1.66f., Duchesne-Guillemin 21f.

[46] Entsprechend scheinen auch, worauf ich hier nicht eingehen kann, synthet. NAg. mit *hu-* nicht frei bildbar gewesen zu sein.

[47] Es sei hier angemerkt, daß sich Adjektivadverbia auf **-s* nicht als eigenständige idg. Kategorie nachweisen lassen. Die beigebrachten Beispiele sind wohl in allen Fällen adverbialisierte Kasusformen, s. Brugmann, Grundriß 2.2.677ff. (zu gav. *maš* s.u.).

[48] Vgl. z.B. die *ἐνθεος*-Komposita.

[49] Ein gutes Beispiel ist das von Bartholomae, IF 9, 283 seiner Bildung nach mit *aš.*- und gav. *maš* verglichene gav. Adverb *ərəš* 'recht, richtig' (ursprünglich wohl NSgm. **h$_3$r̥ĝ-s*), das als KA in den synthet. Komposita *ərəžəjī-* 'richtig lebend', *ərəžuxδa-* 'richtiges Sprechen', jav. *arš.dāta-* 'recht geschaffen' erscheint. Anstelle von **ərəš.vac-* 'richtig sprechend' finden wir jedoch *ərəš.vacah-*, zu dessen jav. Entspechung *arš.vacah-* die parallelen *arš.manah-* 'richtig denkend' und *arš.šiiaoθna-* 'richtig handelnd' hinzugebildet wurden. – M. Mayrhofer weist mich auf den elam.-altpers. PN *Maš-ka-ma* (Onom. Persep. 194 mit Lit.) hin, der wohl als **Maš-kāma-* 'sehr verlangend' zu interpretieren ist. Bildungen mit *-kāma-* konnten also nicht nur als PK, sondern auch als synthet. NAg. fungieren (KN).

[50] Das *-h-* von *máhas-* beruht auf Analogie nach dem Adj. *máh-* $<$ **meĝ-h$_2$-*.

[51] Zu denen mit **-ɥent-* und zum Typ ved. *íṣuhasta-* s. FS Risch 395ff.

[52] Vgl. Eichner, GS Kronasser (1982) 27[60].

The multiple origin of the Indo-European nominative case[1]

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From my early years as a graduate student at the University of Pennsylvania I have admired Henry Hoenigswald, always one of my favorite teachers. I hope that it is not out of place for me to praise not only his scholarship and keen understanding of historical linguistics, but also his qualities of modesty and compassion as a human being. I feel deeply honored to have the opportunity to contribute a small piece to this volume.

1. I have argued elsewhere that Indo-European was originally a language with split ergativity (Schmalstieg 1980: 184–185). In general the forms of the middle aorist and perfect tense were correlated with an ergative syntax and morphology, whereas the forms of the present tense came to be correlated with an accusative syntax. The morphological correlation is expressed by the relationship between the 3rd sg. middle aorist ending **-to*, (cf. Gk. *édo-to*, Old Indic *ádi-ta* 'gave') with the participle in **-tos*, (cf. Gk. *do-tós*, Lat. *da-tus*, Lith. *dúo-tas* 'given', see Hirt 1928: 102 and Schmalstieg 1980: 93). Originally the participle in **-to* was completely intransitive as in Old Indic *ga-tá-* 'gone'. The oldest sentence type would have been represented by (1) **rēģ gʷm̥-tó* 'the king has (is) gone', (2) **rēģ ghʷn̥-tó* 'the king was (is) struck'. The latter sentence must be translated as a passive in English and all of the Western European languages with which I am familiar. Nevertheless at this early stage I interpret it as being without diathesis. The original phrase consisted of only two elements, a subject and predicate which, if verbal, was intransitive. A third element, namely an agent, could be added to such a sentence and one could have (3) **rēģ ghʷn̥-tó gʷenā-s* which we would be tempted to translate as 'the king was struck by the woman'.[2]

This construction, however, is the original Indo-European ergative construction and the passive interpretation only became possible with the appearance of the active voice as outlined in paragraphs 2 and 3 below.[3]

2. At a later period of Indo-European there came to exist sentences of the following type:

(4)	*patér	ghʷent	vl̥kʷom
	father	strikes	at the wolf
	(subject in	(intransitive	(indirect object in
	absolutive case)	verb)	dative-locative case)

'the father strikes at the wolf'. The subject **patér* is in the absolutive case without any marker, the verb **ghʷent* is intransitive and atelic and cannot take a direct object, but rather takes the indirect object **vl̥kʷom* in the dative-locative case. The structure is a

typical antipassive construction known in various ergative languages, with a subject in the absolutive case, an intransitive verb and an (indirect) object in an oblique case. (One notes that in the Bežtinskij language of Daghestan the verb denoting 'to strike' is construed with a dative object [Kibrik 1981: 3].)

3. Within Indo-European in a word-final sequence of short vowel plus sonant the sonant was lost and the preceding vowel lengthened if the following word began with a consonant. If the following word began with a vowel the etymological sequence was retained. Thus the sandhi variants **patér, *vl̥k^{w}om* (prevocalic forms) and **patḗ, *vl̥k^{w}ō* (preconsonantal forms) came into existence. These original automatic variants came to contrast and the meaning of absolutive case was ascribed to **patḗ* whereas the meaning of the vocative case was ascribed to **páter* (with the forward movement of the stress characteristic of this case attested in Gk. *páter* and Old Indic *pĭtar*). The form **patḗ* is represented in the nominatives singular: Old Indic *pitā́* (with the root form **pi-* [rather than **pa-*] typical of Old Indic) and Gk. *patḗr* (with the addition of *-r* from other forms of the paradigm). The new form **vl̥k^{w}ō* retained the old dative-locative function (with the addition of *-y* [*a*]) and the old form **vl̥k^{w}om* became the accusative case, viz., that case which could express a greater degree of verbal transitivity than the dative. Thus sentence (4) is transformed into the following sentence:

(5)	*patḗ	gh^{w}ent	vl̥k^{w}om
	father	strikes	wolf
	(subject in	(transitive	(direct object in
	nominative case)	verb)	accusative case)

The creation of the new accusative case (by splitting from the old dative-locative) was occasioned by an increase in transitivity. The reason for the increase in transitivity is as opaque as the reason for any syntactic change in any language.

Hopper and Thompson (1980: 252) have given a list of items which contribute to the notion of transitivity. Thus a telic verb is high in transitivity; if the object is totally affected it is higher in transitivity than if it is not affected. Timberlake (1977: 141–177) has given an interesting account of an increase in transitivity in Russian in which the accusative case has replaced the old (partitive) genitive as the direct object of negated verbs under certain circumstances. Other examples of an increase in transitivity include the introduction of the particle *z-* to denote a direct object in Armenian and the introduction of the 'personal *a*' in Spanish.

According to Klimov (1973: 193) the origin of the accusative case is to be sought in the indirect case in many Kartvelian languages. In the language Udi, for example, the accusative in *-(a)x* and *-(e)x* shows its origin in the dative case, but only in a reliquary fashion.

In Indo-European, the increase in transitivity led to the interpretation of the antipassive (sentence 4) as an active (sentence 5). When sentence (5) became a true active then sentence (3) could only be interpreted as passive.

4. Already Brugmann (1911: 122–123) had divided up the Indo-European nominatives singular into (1) endingless forms, i.e., the *-ā, -(i)iə̄-: -ī-, -n, -r, -s* stem forms and (2) forms with the ending *-s*, i.e., the *-o, -i, -u, -ī, -ū*, dental, guttural (velar in our terms) stems and root nouns.

As in my (1980: 183–185) work I still see the endingless forms as reflecting the old absolutive case without any marking at all. Typically the subject case of an intransitive verb in an ergative language is unmarked. The passage of the antipassive unmarked subject in the absolutive case (typical of an ergative language) to the unmarked subject in the nominative case (typical of an accusative language) is quite comprehensible.

The problem is to explain the *-s* of the nominative case. Earlier I had attempted to explain this as the relic of a verbal-adjective formation derived from a stage prior to the separation of adjective in (nom. sg.) **-os* from the verb in **-es* (3rd sg. active < antipassive). Thus (6) **patér bhor-os* 'father (is) a carrier, porter' vs. (7) **patér bher-es* 'father (is) carrying'. The **e/os* denotes the animate pronoun whereas **-e/ot* denotes the inanimate. In what came to be the verb usually **-et* was generalized in the third singular, while **-es* was generalized in the second singular. Relics with **-s* in the third singular are, however, encountered, e.g., Tokharian A 3rd pres. *pälkäṣ* 'lightens', Old Persian *aiš* 'went', Phrygian *edaes* 'put' (Schmalstieg 1980: 103).

The formal identity of the Hittite nom. and gen. sg. *an-tu-uḫ-ša-aš* 'man' has led many to identify the **o*-stem genitive singular ending with an etymological ergative case. One analyzes then Old Indic gen. sg. *vīr-as-ya* as deriving from **vīr-as* plus a particle **io* (Ivanov 1959: 28; Mažiulis 1970: 91) or a relative **yo* (Lehmann 1981: 179–188). One could suppose that a sentence such as (3) at the earliest period, may have had the agent **vīros* 'by the man'. Thus:

(8)	*rēĝ	ghʷn̥-tó	vīr-o-s
	king	was struck	by the man
	(subject in absolutive-nominative case)	(intransitive verb)	(agent in ergative-genitive case)

'The king was struck by the man'.

5. Briefly digressing, note now the following old ergative sentence from Lithuanian (Paulauskienė 1979: 105):

(9)	Añt šakẽliu	ant	žaliūjų	vainìkai	kabìnta
	On branches	on	green	wreathes (nom. pl. subject)	(are) hung (neut. sg. past passive participle)

'Wreathes are hung on the green branches'.

In modern Lithuanian the nom. pl. *vainìkai* can be replaced by the acc. pl. *vainikùs* thus changing the syntactic relationships and making *vainikùs* the direct object of the verb.

The Lithuanian example furnishes a parallel with the Indo-European situation in which sentences of type (8) with a nominative subject are replaced by sentences with an accusative object.[4] The passive interpretation of the verb was then replaced by an active interpretation.[5]

(10)	*rēĝ-m̥	ghʷn̥-tó	vīr-o-s
	king	struck	man
	(direct object in accusative case)	(transitive verb)	(agent in genitive-ergative > nominative case)

'The man struck the king'.

Thus sentence (10) becomes parallel to sentence (5) and the agent in **-s* becomes parallel to the absolutive case without any **-s*. The **-s* spread in the nominative case to the extent that it was felt necessary to mark the agentivity of the nominative case. Suffixes with high agentivity such as **-ā, *-ter*, etc. did not require the **-s*.

The penetration of the ergative into the absolutive case function is known, e.g., in western Georgian dialects (Boeder 1979: 443). Here one encounters *k'ac-ma movida* 'the man came' and *k'ac-ma mok'vda* 'the man died' where the ergative ending *-ma* has replaced the nominative *-i* which standard Georgian uses for the subject of such intransitive verbs.

I suggest furthermore that the introduction of the **-s* into the nominative case was an ongoing process which was never, indeed, completed. One notes that in Hittite the personal names are sometimes supplied with a final *-š* and sometimes not. Thus one encounters the nom. sg. *la-ba-ar-na-aš* 'king' (deriving from an old proper name) as well as *la-ba-ar-na* (Güterbock and Hoffner 1980: 41) without *-š*, similarly *Mur-ši-li* and *Mur-ši-li-iš*, etc. As Čikobava (1948: 223) has pointed out, in Old Georgian the indefinite case was originally used for agent with proper names and one encounters *Abraam šva Jsaaḳ* 'Abraham bore Isaac' with *Abraam* in the indefinite case rather than the expected ergative *Abraam-man*. On Silverstein's agentivity scale the proper nouns are more likely to function as a transitive agent than common nouns, so the marking with the **-s* was later in the proper names (Dixon 1979: 85).

Schwyzer (1966: 64) writes that in Greek the inanimate masculines, feminines and neuters were limited to the function of subject and predicate in nominal sentences and to the function of subject of stative-fientive intransitives and passives, whereas animate masculines, feminines and neuters (thus *téknon* 'child' or collectives like *stráteuma* 'army, host') could function as the subject of verbs of action (the so-called *energetica*), which were mostly transitive. Tchekhoff (1978) quoting Laroche (1962) and Benveniste (1962) has noted that in Hittite an inanimate noun can be the subject of an intransitive verb or the object of a transitive verb, but not the subject of a transitive verb. Thus, for example, the neuter word *paḫḫur* 'fire' can be the subject of the intransitive verb *warani* 'burns'. On the other hand, in order for the noun 'fire' to become the subject of a transitive verb it must take the ending *-s*, e.g.:

[man]	antuḫš[an]	ḫuwaḫḫ[urtin]	p[aḫ]ḫuenaš	epzi
when	man	throat	fire	seizes
	(acc. sg.)	(acc. sg.)	(nom. sg.)	

'When fire seizes a man by the throat' (Tchekhoff 1978: 231).

There is some doubt as to whether this is a nominative or a genitive form, but even if it is a genitive form, it would illustrate the principle that the more agentive nouns rendered the active voice more transitive.

Thus the Indo-European endingless nominative cases derive from the old absolutive case and the sigmatic nominative derives from the old ergative (or perhaps, if my earlier notion is correct) from the old intransitive adjective-verb. The difference in the syntactic relationships represented in the active voice (< antipassive) penetrated into the middle and perfect tenses and Indo-European shifted from a language with split ergativity to an almost completely accusative language (remnants of the old ergativity only being retained in participial constructions in **-to* in Baltic, Slavic, Old Indic, Greek, etc.). The endings of the

middle voice and perfect tense are much more difficult to reconstruct than those of the active voice. The reason is clear. Although the middle voice and perfect tense are historically older than the active voice, their syntax and morphology have been completely reshaped by the latter.

Notes

[1] I would like to thank Philip Baldi for suggestions and references, particularly in regard to the classical languages. Naturally I assume full responsibility for the contents of the paper.

[2] In Lithuanian passive constructions the genitive case denotes agent as opposed to the instrumental which denotes instrument, so that an expression such as *parašýta màno* 'written by me (gen.)' contrasts with *parašýta pieštukù* 'written with a pencil (inst.)'. One can assume an original ergative-genitive agent in Indo-European also, cf., e.g., Gk. *Diós-dotos* 'given by Zeus' and for Old Indic see footnote 6 below. Further evidence is given in Schmalstieg 1980: 176–178.

[3] Indeed sometimes it is difficult to make out the interpretation in Greek. Thus Schwyzer (1966: 237) writes that the phrase (N 597) *tò d'ephélketo meílinon égkhos* could be interpreted either as 'the ashen spear dragged after' (*der eschene Speer schleifte nach*) or 'was dragged after' (*wurde nachgeschleift*), or even 'he dragged the ashen spear after himself' (*er schleifte den e. Sp. hinter sich nach*). Likewise (A 5) *Diòs d' eteleíeto boulḗ* could mean 'the will of Zeus was going towards fulfilment' (*ging der Vollendung entgegen*) or 'was being fulfilled' (*vollendete sich* or *wurde vollendet*).

[4] Such examples as the following are usually explained as being accusative of relationship, but I propose that they could be explained as reflecting the activization of a passive construction, i.e., the substitution of an accusative for a nominative. From the Iliad: (PS 777) *en d'ónthou boéou plḗto stóma te hrῑ̃nas* 'and with the filth of bulls were his mouth and nostrils filled'. (E 354) *melaíneto de khróa kalón* 'and (her) fair flesh was darkened'. The Greek examples fit exactly with the proposed theory and one can compare the relationships in (PS 777) with those of Old Armenian as explained below in footnote (5) by Benveniste. Note even the genitive case of *ónthou boéou* which originally functioned as ergative agent. In Latin such examples include particularly verbs denoting 'putting on or taking off clothing, adorning, crowning, covering, etc.' (Kühner-Stegmann 1962: 288–289). Thus (Ov. M. 1, 270) *varios induta colores* 'clad in robes of many hues', (Verg. E. 6, 68) *floribus atque apio crines ornatus* '(his) hair adorned with flowers and with bitter parsley', (Verg. A. 12, 416) *faciem circumdata nimbo* '(her) face veiled in dim mist', (Verg. A. 8, 457) *tunicāque inducitur artus* 'and (he) clothes his limbs with a tunic'. Kühner-Stegmann (1962: 285) write that the Latin accusative of relationship must be under Greek influence since it only appears in Augustan time in Latin, originally in poetic language, particularly in Virgil and Ovid and in prose only in Tacitus. I would not care to give an opinion on all the examples, but I see no reason why the same syntactic forces could not have been at work in Latin as in Lithuanian (and apparently Greek), namely the substitution of an accusative for an old nominative. See also Gołąb (1975: 15).

[5] A somewhat similar phenomenon is observed in Old Armenian where the accusative replaces the old nominative. Benveniste (1971: 159) writes "In other words, *z-gorc gorceal e nora* 'he has accomplished the work' does not mean *'eius facta est opera'* but *'eius factum est operam.'* Since *'eius factum est'* is the equivalent of *'habet factum'*, it is not surprising that *'eius factum est'* adopts the transitive government of the earlier *fecit*, which it replaces in Armenian, and that it requires a determined object."

[6] If, however, the **-to* of the verb was used as a participle and there was verbal agreement between the subject and the participle the agent remained in the genitive case and the syntactic change did not take place, cf. Old Indic:

pátyuḥ	krītā́	satī́
by the spouse	bought	wife
(gen. sg.	(fem. nom. sg.	(nom. sg. fem.
agent)	past passive participle)	subject)

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An Indo-Iranian etymological kaleidoscope

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1. Sanskrit *darad* 'heart'

In his commentary on Uṇādisūtra I 129 Ujjvaladatta gives 'heart' as one of the meanings of the word *darad*. Since there is no literary attestation, it is of course impossible to tell whether the meaning is correctly reported or based on a misunderstanding of the source. The word is listed as a derivative of the root *dṝ* 'to split, tear asunder'. Semantically this etymology is implausible. If the word is real at all, it is possibly of foreign origin. Thus the Old Persian word for 'heart', **dərəd*, suggests itself. If this is correct, the borrowing occurred at a time when the change to Middle Persian *dil* had not yet taken place. The ultimate literary source from which Ujjvaladatta's information stems may then be 1500 or more years earlier than he himself (end of 13th century A.D.).

2. Sanskrit *darada* 'red lead'

The shorter Petrograd dictionary quotes *darada* n. from the Bhāvaprakāśa in the meaning 'red lead'. A derivation from *dṝ* is unlikely. A colour term would be a more probable candidate. In genuine Old Persian we have *daraniya* 'gold' and would expect **darita* as the equivalent of Avestan *zairita* 'yellow, yellowish red'. The colour range covered by derivations of the base *zar* extends from yellow over green to red (cf. Bailey 1974: 372). Firdosi used *āb i zard* of blood (cf. Lentz 1926: 312). In Middle and New Persian we find only initial *z* in the derivations of the base *zar*. The genuine Persian forms, **dar* etc., were replaced by the common Iranian ones, possibly because of the numerous homophonic clashes. The continuation of **darita, *dard*, clashed with *dard* 'pain'. It is however possible that the old South Western form was partially retained, either dialectally or in a specialized term.

3. Sanskrit *kuḍaka* 'child'

In Sanskrit the word *kuḍaka* is so far attested only once in the Kashmiri tradition of the Mahābhārata 6.2.8. The correct meaning, 'child', was recognized by Burrow (1957: 6) and Agrawala (1959: 30). Burrow based his interpretation on the North Western Prakrit *kuḍaga*, fem. *kuḍi*, of the so-called Niya documents. At one time the word must have had wider currency in Sanskrit since the Dhātupāṭha VI 89 lists a verb *kuḍáti* in the meaning *bālye*

'acting like a child'. In his glossary (1937: 83) Burrow stated: "A connection with N. Pers. *kūdak* 'child' is not out of the question." Later he preferred to derive the word from Dravidian (1943: 135 n. 3). Tamil has *kuṛa* 'young', Kanarese *koḍa* 'youth'. In Munda languages homophonic words also occur, thus Santali *koṛa* 'boy', *kuṛi* 'girl'. Burrow suggested that the Munda words derive from Dravidian through Indo-Aryan mediation. Turner (1966: no. 3245) leaves it open whether the ultimate origin is Munda or Dravidian. With the sole exception of Bengali *ā̃ṭ-kuṛā* 'childless' the possible Modern Indo-Aryan cognates listed by Turner are restricted to North Western India, and this fact makes Dravidian or Munda origin somewhat doubtful. The Iranian connection should therefore be reconsidered.

From Niya Prakrit *dhane* < Iran. **dānaka* '4th part of the drachma', *ajhate* < **āzātaka* 'free' and *saste* < **sastaka* 'day' Burrow (1935: 783; cf. 1937: 99) concluded: "It appears that in Iranian words original *-aka* is represented by *-e* in this language." If this were a strict rule, one would be tempted to infer from it that in the Middle Iranian source language final *g* < *k* was already dropped. This would however be a fallacy since the development *-aka* > *-aǵa* > *-ae* > *-e* is also otherwise attested in Niya Prakrit. Burrow (1937: § 53) mentions the last stage in the discussion of the declension, but omits it in the phonology (§ 8; 16). Burrow's 'rule' can consequently not be used as an argument against the possible Iranian origin of *kuḍaǵa*.

The Iranian form will have been **kuδak* or **kuδag*. The vocalism of the first syllable agrees with Avestan *kutaka* 'small'. Pahlavi *kwtk*, Manichaean Middle Persian *qwdk* and New Persian *kōdak* 'young, boy' show guṇa-forms. It cannot be determined whether the final consonant was *k* or *g* since we do not know whether the source dialect had preserved the tenuis of the diminutive suffix as New Persian did (cf. Horn 1898: 64).

The Iranian words derive, as Bailey (1954: 144f.) has shown, from an Indo-European root **kau : ku* 'to be small'.

4. Ṛgvedic *kumārádeṣṇa*

Bailey (1954: 144f.) analyzes *kumārá* 'boy' as *ku-ma-āra* from the root *ku* 'to be small', and Burrow (1955: 187f.) accepts this explanation. It is much superior to Böhtlingk's etymology, 'easily dying > perishable > tender'. Böhtlingk found the only adjectival use of *kumārá* in the bahuvrīhi *kumārádeṣṇa* (Ṛgveda 10.34.7), an epithet of the dice. Since the gains one makes with dice do not perish but are rather lost again, the interpretation 'whose gifts are perishable' is not very plausible, and Geldner proposed in his glossary the translation 'whose gifts are those of children', i.e. the gifts once given are taken away again. As Oldenberg emphasized, the phrase following the word in the text strongly supports this interpretation: *jáyataḥ punarhánaḥ* 'beating again the winning one.' Neisser (1930: 61f.) argued that claiming gifts back is not a characteristic behaviour of children, and he defended Böhtlingk's translation. Burrow maintains that Geldner's interpretation presupposes an abnormal bahuvrīhi, and he prefers 'whose gifts are small'. There is however nothing wrong with Geldner's dissolution of the compound (cf. Wackernagel-Debrunner 2/1 § 101 for parallels). Moreover, in the whole history of Sanskrit, Pali and Prakrit *kumāra* is always a substantive, never an adjective, which agrees with the use of the suffix *-āra* (Wackernagel-Debrunner 2/2 § 174). As to Neisser's criticism, based on the opinion of a psychologist, it may be said that even though taking gifts away again is not habitual with children it is of frequent enough occurrence to make it appear to be a characteristic trait.

5. Ṛgvedic *kavatnú, kavāsakhá, kavārí* etc.

Hoffmann (1957/1976: 411f.) has convincingly argued that *kavatnú* presupposes a present stem **kavati* and that *kavāsakhá* and *kavārí* are compounds of the type *śikṣānará* 'giving gifts to men'. *kavāsakhá* occurs as an antonym of *maghávan* 'generous, liberal' in Ṛgveda 5.34.3, and the meaning generally assigned to it, 'stingy towards the companion', is plausible. The *kavārí* in 10.107.3 are characterized by the words *nahí té pṛṇánti* 'for they do not give gifts', which clearly defines them as 'stingy to the stranger'. This is acknowledged by Hoffmann. It should be added that in 7.32 the pāda 9d *ná devāsaḥ kavatnáve* stands in obvious opposition to pāda 8d *pṛṇánn ít pṛṇaté máyaḥ* 'only the giving one is a joy to the giving one' which determines the meaning of 9d as 'the gods (are) not for (= in favour of) the stingy one'.

In his search for an Indo-European root which can be connected with the reconstructed Vedic **kavati* Hoffmann finds **kau* 'to humiliate; disgrace; shame' most suitable. This root has been posited on the basis of Germanic and Baltic material. Hoffmann wants to interpret the Vedic words accordingly. Although it is conceivable that one humiliates somebody by stinginess, it is not very plausible to assume that terms which literally express the humiliation acquire the sense of stinginess.

Hoffmann became acquainted with Bailey's and Burrow's reconstruction of a root **kau : ku* 'to be small' only after he had formed his own hypothesis. He concedes that the Indo-European **kau* appears to have had the concrete meaning 'to make small, inferior', but by restricting it to the factitive sense he demonstrates his bias. He is also at pains to stress that Pahlavi *kwtk* 'small' has also the sense 'lowly, humble' and thus means the same as Gothic *hauns* 'lowly'. Furthermore he recognizes with Herzfeld Old Persian *skauθi* 'lowly, poor' and Pahlavi *škwh* 'poor' (vṛddhi-formation of **sku-θa*) as cognates of *kwtk* etc. In Pahlavi *kw't(k)* 'young (of man or animal)' he finds the substantive *kava* which is also present in Vedic *ákava*. While this is formally unobjectionable, semantically the interpretation of *kava* as 'Erniedrigung, Beeinträchtigung, Minderung' is unconvincing. Only in very special contexts would one speak of the young as being characterized by 'diminution' rather than simply by 'smallness'.

'Deficient, lowly, humble, poor, miserable, despised' are universally attested secondary connotations of words for 'small'. 'Disgrace, shame' are also easily derivable from a basic meaning 'to be, become, feel small'. Only Gothic *haunjan* 'to make lowly, humiliate, degrade' and Old High German *hōnen* 'to scorn' show factitive meaning which is however best explained by the fact that these verbs are denominatives. That Germanic and possibly also Greek (if καῦρος ·κακός belongs here) have preserved only pejorative connotations, is a special development. In Baltic, Lettish *kaũns* 'disgrace, shame', Lithuanian *kuvėtis* 'to be ashamed' show the pejorative sense while Lithuanian *kumelỹs* 'foal' and *kumẽlė* 'filly' continue the concrete meaning 'to be small'. In Indo-Iranian there is no trace of the factitive sense 'to humiliate'. The words *kavatnú, kavāsakhá* and *kavārí* express stinginess which is frequently associated with smallness as demonstrable by English *petty* and German *kleinlich*.

Since humiliation does not belong to the connotations of the root *ku* in Indo-Iranian, Hoffmann's suggestion (apud Mayrhofer 1953–1980: III 677) to explain the Old Persian name *Kuru* as 'humiliator of the enemy in the verbal contest' must also be rejected. The evidence

Werba (1980: 16ff.) adduces from Xenophon's Cyropaedia in support of Hoffmann's hypothesis is fallacious: it would have probative force only if it showed that the name of Cyrus was associated with bragging and singing the battle-song. Mayrhofer (1979: II 23) is right in leaving the question of the etymology of *Kuru* open since even the nowadays most widely advocated connection with the root *ku* 'to be small, young' (cf. esp. Szemerényi 1977: 13ff.) lacks the corroboration required. Incidentally it may be mentioned that Wüst's (1966: 77) attempt to show that 'young dog' is the original meaning of *Kuru* meets with the same objection as Werba's argument: our sources do not associate the name of Cyrus with his having been nursed by a dog.

With Hoffmann Vedic *ákava* is best explained as a bahuvrīhi. Its literal meaning is 'without smallness, deficiency', and it is used as a litotes; Geldner's translation 'vollkommen' = 'perfect' is quite adequate. Hoffmann also explains *kavā* in *kávātiryañc* 'directed a little across' convincingly as an instrumental of the substantive *kava*.

Burrow (1955: 189ff.) also places here the common prefix *ku* which was generally regarded as a form of the interrogative pronoun (Wackernagel-Debrunner 2/1: 82) because from the later Vedic language onwards it alternates with other forms of the interrogative (*ka, kad, kim*). Pāṇini took the pejorative sense of *ku* as the basic one as can be inferred from his sūtras 6.3. 105ff. where he teaches the substitution of *ku* by other forms when the sense *īṣat* 'a little' is intended. Historically the development was the other way round: *ku* 'little' also assumed a pejorative sense which became predominant though not exclusive. According to Burrow the interpretation of *ku* as an interrogative is secondary and has triggered the alternation with the other forms. Burrow states that outside Indo-Aryan such a use of the forms of the interrogative pronoun is not attested. He does not discuss the parallels which have actually been adduced.

Obviously he rejects, as Charpentier (1924: 14) had done before him, the parallels which Bartholomae believed to have found in Avestan (cf. the complete list in Duchesne-Guillemin 1936: § 183). In Avestan *kū.nāirī*, generally translated by 'harlot', Burrow finds the pejorative sense of 'small'. This is plausible. It should however be added that the word is, in its only attestation, used of a homosexual (Vīdēvdād 8.31–32) and probably had not the sense of 'harlot', but that of the partner acting the female part in homosexual relationships, and consequently the word is best understood as a 'deficient woman' because the homosexual is not a real woman.

There is another word which may contain *ku*: *kuruγa*. If this is the name of a disease, which is by no means certain, it could be connected with Sanskrit *róga* 'disease', as Schwyzer (1929: 113) suggested. 'Bad disease' would be suitable, but it is uncertain whether the existence of the root *rug/ruj* can be presupposed in Iranian (cf. Mayrhofer 1953–1980: III 64 s.v. *rujáti*). On the Avestan words with initial *ka* see the following note.

The most striking and persistently quoted parallel from outside Indo-Iranian is the Boeotian πούλιμος (Old Boeotian πύλιμος) 'awful hunger' which Schulze (1895/1933: 399f.) separated from Greek βούλιμος for phonetic reasons. For βούλιμος and other compounds with βου as intensifier he accepted the connection with βοῦς because the Greeks themselves recognized it. He interpreted the Boeotian prefix πυ as the equivalent of Sanskrit *ku* following the interpretation of the Petrograd dictionary according to which it originally had the function of emphasizing the extraordinary, unusual aspect of a phenomenon. He deemed this isolated example as sufficient evidence to claim Indo-European age for this peculiar

type of compound. There have been few to take issue with this ingenuous but daring assertion. Meillet (1916: 349) said that there is no reason to separate *βούλιμος* from *πούλιμος* because in Slavic the alternation between *ka* and *ga* occurs in similar compounds (see also the next note). This is a perverse argument since alternations in one language family cannot prove anything for alternations in another family. From the context one must however infer that Meillet approved of Schulze's etymology and rather wanted to separate the prefix *βου* from the name of the bovine. Bechtel (1921: 246f.) pointed out that one would expect rather *κυ*, *κου* in Boeotian, but accepted Schulze's interpretation in spite of this serious obstacle. Charpentier (1924: 14 n.1) rejected it without giving any reason. Chantraine (1968ff.: I 187) still quoted it with approval. In my opinion the phonetic difficulty is serious enough to reject it. If the alternation between Greek *βου* and Boeotian *που* cannot be justified by any analogies, another derivation of the Boeotian word may be considered: the prefix *πυ* could be connected with the adverb *πυ*, attested in the superlative *πύματος* 'extreme'. This suggestion is offered for what it is worth: the origin and exact meaning of the adverb *πυ* are after all not definitely established (cf. Frisk 1952–1970: II 624).

Other alleged parallels to the use of the interrogative in Sanskrit compounds are from modern languages and can hardly be claimed as proof for the Indo-European age of the compound type. Thus we find in Byelorussian *čymalyĭ* 'rather big, considerable' "is that supposed to be small?!" (Fraenkel 1923: 243). Such words probably belong to those 'Augenblicksbildungen' which happen to attain wider currency like the English *whatnot*.

6. Avestan *kamərəδa* and the problem of the prefix *ka*

Taking it for granted that the Avestan word for the head of a daēvic, demonic creature, *kamərəδa*, is related to Sanskrit *mūrdhán* 'head', Bartholomae (1904: 440) explained the prefix *ka* as the interrogative in a pejorative sense: 'what a head!' = 'a hideous head'. The other examples which Bartholomae claims for this type are all words of negative sense: *kaxvarəδa* 'magician', fem. *kaxvarəiδī* (cf. the loanwords in Armenian *kaxard* '*γόης*, *φαρμακεύς*', Sanskrit *kākhorda, kakkhorda, kharkhoda, kharkhoṭa* 'a kind of magic'; possibly also Niya Prakrit **khakhorda* according to Burrow (1935: 781; 1937: 86)); *kaxužī* 'an evil female'; *kapasti* 'a certain infectious disease'. Bartholomae's etymologies of these words are far from convincing. To connect *kaxvarəδa* with Gothic *swarts* 'black' is a hazardous guess since relatives of the Gothic word are lacking in Iranian. The same objection applies to the collocation of *kapasti* with Latin *pestis* which is of unknown origin. *kaxužī* could be related to Sanskrit *kuhaka* 'trickster, juggler', but the latter word can also be derived from *kuha* 'where' (Mayrhofer 1953–1980: I 249).

Schwyzer (1929: 111f.) added *kayaδa* to the list. He assumed a borrowing from Old Persian which has *yad* 'to worship' against Avestan *yaz*. *kayaδa* would then mean 'how (= badly) worshipping' and by the dialectal form also allude to the pronunciation of the people performing the outlawed worship. In spite of Duchesne-Guillemin's attempt (1936: § 183) to support this hypothesis, it has little to recommend itself. There is no evidence which allows us to single the Persians out as arch-devil-worshippers. The relation between *kayaδa* and *kāiδya* does not favour this etymology (cf. now also Dehghan 1982: 70 for the suggestion to interpret *kāiδyå* as gen. sg. of the fem. *kayeiδī*). An Old Persian word *kayādā*, which is doubtlessly related, has become known in the meantime (Steve 1975: 10f.). It

occurs in juxtaposition with *yātūm* (= Avestan *yātu* 'sorcery') and is probably a neuter plural form (differently Mayrhofer 1978: 30f.). The Akkadian version has: 'may (the gods) neither damage nor destroy what I have built'. In the Old Persian version the verb is not preserved, only the initial *vi* and the imperative ending *-itu*. Probably a verb meaning 'to meet with, fall into' has to be restored: 'let what I have built not meet with damaging sorcery, not with destruction'. Although we cannot expect an exact translation in the Akkadian version, it shows that the destructive force of *yātu* and *kayāda* was felt as dominant. This agrees with the Pahlavi translation of Avestan *kayaδa* : *kastār* 'diminisher, destroyer'. Also *kaxvarəδa* and *kaxužī* were interpreted as *kastār*, but specifically of *xvarrah* 'fortune'.

In all probability *kayaδa* has to be dropped from the list of words possibly containing the prefix *ka*. If the other words already had a negative sense in their uncompounded forms, the pejorative meaning of *ka* cannot be ascertained.

This must be emphasized also in view of the sweeping assertions of Meillet (1916: 348f.) who accepted Bartholomae's theory without reservations. As to *kamərəδa* he went even so far as to maintain that this Avestan word with its short root vowel, contrasting with the long vowel in Sanskrit *mūrdhán*, suffices to show that the constitution of two different vocabularies for good and evil beings is very ancient because the loss of the (laryngeal) *ə* (which caused the length in *mūrdhán*) in noninitial position goes back to high antiquity in Iranian. Pahlavi *kamāl* < **kamarda* presupposes the existence of the uncompounded word in Iranian. Furthermore Meillet adopts the theory of Miklosich that a Slavic word for the raven, Serbian *kàvrān* etc., contrasting with Old Slavic *vranŭ*, is a compound of the type like Avestan *kamərəδa*. He does not spare a word about the question of whether Serbian *kàvrān* is an evil raven and Old Slavic *vranŭ* a good raven. There is a distinction between *vóron* 'raven' and *kávoron* 'rook' in Russian, but it is certainly not one of good and evil. Even if the purely linguistic part of Meillet's argument is accepted, the conclusions as to the differentiation of the vocabulary are more than hazardous.

Charpentier (1924: 12ff.) rejected Bartholomae's theory and proposed *kamərəδa* with Sanskrit *kamaṭha* 'tortoise; begging-bowl of an ascetic'. Although semantically there is no difficulty in associating the shell of the tortoise with the skull – as Latin *testa* > French *tête* and Sanskrit *kapāla* show –, there remain phonetic obstacles which can be overcome only by complicated hypotheses.

A weighty argument against Bartholomae's theory can be derived from Khotanese *kamala* (< **kamṛda*) 'head' in a non-pejorative sense. From the Pamir dialects Bailey (1979: 52) quotes Yidγa *kyɛmalγo* 'skull'. The differentiation of daēvic and ahuric vocabularies in the Avesta is not a homogeneous system. In the case of *kamərəδa* it is not possible to tell whether it already had an exclusively pejorative sense when the religious differentiation was introduced. Its ahuric counterpart, *vaγδana*, is of unknown etymology and has no certain cognates in other languages. It is hardly a learned word as Bartholomae thought since then one would expect rather a term of transparent etymology as we have it in *paitīštāna* 'leg'. I would prefer to see in it an archaic word like *staman* 'mouth'. *kamərəδa* may have had the restricted sense of 'skull' and thus offered itself readily to demonization. There is another word for 'head' in Avestan, *sarah*; it was probably the *vox media* because it continued in this use in several Iranian dialects, among them Persian.

I am quite aware of the fact that negative terms can undergo an amelioration. That this was the case with Khotanese *kamala*, is however a rather remote possibility.

Pisani (1954: 125f.) draws attention to Bartholomae's earlier proposal (1895: 224ff. cf. also Bloomfield 1895: 423) to explain *kamərəδa* as a "proportional formation" to the Iranian equivalent of Sanskrit *kakúbh* 'peak' (Latin *cacūmen*) if this can be connected with Cretan κυφή 'head'. Pisani picks up Fick's collocation of *mūrdhán* with Greek μέλαθρον 'ceiling, roof, house' and sees in the variant κμέλεθρον the etymological equivalent of *kamərəδa* whose *a* he explains as a mere anaptyctic vowel. *k* is for him a velar 'praeformans' also found in Slavic *kosti* 'bone', Latin *costa* 'rib' besides *os* 'bone' and other words. While in some cases the praeformans is accompanied by a change of meaning, in others it is not. It remains an enigma.

As befits the Ahrimanian pate, *kamərəδa* will continue to confuse scholars.

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Altpersisch *m-n-u-vi-i-š̌* = *manauvīš̌*

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> "Eine Etymologie, die nichts weiter als eine linguistische Möglichkeit ist, gleicht einem Kartenhause, denn die nächste linguistische Möglichkeit, die bekannt wird, bläst sie um."
> (Friedrich Bechtel, Lexilogus zu Homer, 1914, S. V)

Zu den historisch bedeutsamsten altpersischen Königsinschriften der Achaimeniden gehört zweifellos der berühmte 'Fürstenspiegel'[1] des Dareios, die Inschrift DNb. Die Zahl der mit diesem Text verknüpften vielfältigen Probleme hat durch den überraschenden Neufund einer nahezu textgleichen Xerxes-Inschrift[2], XPl, allerdings eher zu- als abgenommen. Nichts Neues ergab sich daraus für das in diesem Text(paar) aufscheinende Hapax legomenon *m-n-u-vi-i-š̌*, das in dem kurzen Satz aus der Selbstcharakteristik des Königs DNb 13 vorkommt[3]:

. . . *n-i-y* : *m-n-u-vi-i-š̌* : *a-mi-*[*i-y*] . . . = *naiy manauvīš̌ am*[*iy*],

zu deutsch etwa: 'Ich bin nicht leidenschaftlich/heißblütig/rachsüchtig/jähzornig/unbeherrscht'[4]. Im großen und ganzen ist dabei durchaus klar, was mit diesem Adjektiv gemeint sein muß. Die genaue Bedeutungsnuance läßt sich allerdings höchstens von Morphologie und Etymologie her feststellen, ergibt sich doch auch aus den Parallelversionen kein ganz eindeutiger Hinweis: Die elamische Entsprechung ist]*-iz-za-ma-in-da*, das von Hinz (1969: 56b und 61b) zu [*te*]*-iz-za-ma-in-da* ergänzt wird und Wiedergabe von altiran. **taiǰavant-* o.ä. 'mit Schärfe versehen' (o.ä.) sein soll[5]; doch verbleibt dies völlig im Unverbindlichen. Demgegenüber steht im Babylonischen jedenfalls leidlich klares *ul mam-ma šá i-ga-a-ga ana-ku*, etwa 'Ich bin nicht einer, der [scil.: leicht] zornig wird/in Wut gerät' (Hinz 1969: 47a und R. Borger *apud* Hinz 1969: 57a).

Es gilt also zu fragen, wie die Form altpers. *manauvīš̌* – die einzige Ambiguität in der Graphie betrifft die Quantität des letzten Vokals – genau zu interpretieren ist und ob man nicht über das Fazit von Brandenstein-Mayrhofer (1964: 131) "Die Bildung bleibt unklar" hinauszukommen vermag. Dabei ist zunächst zu betonen, daß, da zweifellos ein Singular-Nominativ vorliegt, die Form als *-i-*stämmig (Typus *Fravartiš̌*) oder als *-ī-*stämmig aufgefaßt werden muß. Für letzteren Fall wäre, *argumenti causa*, zu rechnen (1) mit suffixalem *-ī-* und Flexion entsprechend dem vedischen *vṛkī́*-Typus (im Altpersischen unbezeugt[6]), (2) mit suffixalem *-ī-* und Flexion entsprechend dem vedischen *devī́*-Typus, jedoch moder-

nisierender Umbildung des Nominativs *-i-y* /-ī/ zu *-i-š* /-īš/ innerhalb des Altpersischen (Typus *Uvārazmīš* neben *Uvārazmīy*)[7] und (3) mit wurzelhaftem *-ī-* etwa in einem Wurzelnomen, das das Hinterglied eines Kompositums bildet.

Mit der Konstatierung dieser Fakten ist bereits gesagt, daß die etymologische Verknüpfung von altpers. *manauvīš* mit ved. *manasvín-* 'sinnvoll, verständig, klug', die zuerst Kent (1939: 170) vorgeschlagen hat[8], zum Scheitern verurteilt ist. Zwar könnte man die Umbildung des Nominativs (ved. *manasví* =) altpers. **manauvī* zu *manauvīš* in Analogie zu dem entsprechenden Vorgang bei den im Singular-Nominativ gleich endenden Ländernamen des *devī*-Typus im Altpersischen (vgl. oben) – Kents Operieren "with added *-s*" (Kent 1939: 170; Kent 1950: 65a § 187) für **manasvī/manauvīš* ist seltsam widersprüchlich – nach den 'Regeln der Kunst' allenfalls noch plausibel machen. Aber dann wären immer noch die mindestens ebenso gewichtigen Bedenken aus dem Weg zu räumen, daß (1) die (eher negative) Bedeutung von altpers. *manauvīš* keinesfalls mit der (eher positiven) des angeblichen vedischen Pendants *manasvín-* zusammenpaßt und daß (2) der angenommene Stamm altpers. **manauvin-* der einzige Beleg für ein solches Suffix *-vin-* im Altpersischen wäre (vgl. Kent 1950: 52b § 155.V), ja – schlimmer noch – daß überhaupt "außerindische Entsprechungen zu *-vín-* ... nicht sicher nachgewiesen" sind und die "nahe Beziehung von *-vín-* zu *-vant-* ... und *-ín-* ... den Gedanken nahe(legt), daß *-vín-* eine nach *-ín-* erfolgte Umbildung von *-vant-* ist"[9], mithin also eine indoarische Neuerung vorliegt, für die ein iranisches Gegenstück gar nicht zu erwarten ist.

Mit dem einzigen weiteren in der Fachliteratur diskutierten Erklärungsvorschlag zu diesem Wort, dem von Herzfeld (1938: 242f.), steht es jedoch nicht besser. Herzfeld hatte an ein *-i*-Adjektiv zum Präsensstamm **manau-* gedacht, wie er im Iranischen in, wie er schreibt (Herzfeld 1938: 242), "manauϑrī-, fem. zu *manaᵘtar- 'mentor, mahner'" – gemeint ist damit altavest. *manaoϑrī*, fem. 'gemahnend, erinnernd an etw.' – vorliegt. Den entscheidenden morphologischen Einwand gegen ein solches Verständnis der Form, daß es nämlich derartige *-i*-stämmige Adjektive zu Präsensstämmen (und speziell solchen auf **-au-*) sonst nicht gebe, hat bereits Kent (1939: 170) erhoben[10]. Dies läßt sich für das weitaus reichere Belegmaterial des Altindoarischen jetzt auch an Hand von Wackernagel-Debrunner (1954: 291–307 §§ 185–193) leicht überprüfen.

Da die bisherigen Versuche nicht zu einer überzeugenden Interpretation von altpers. *manauvīš* geführt haben – so schon Brandenstein-Mayrhofer (1964: 131) –, mag es geboten erscheinen, im Rahmen der oben abgesteckten Grenzen einen anderen Weg zu beschreiten: Ich verstehe diese Form als *manauvīš* < iran. **manah-vī-š* und als regelgemäße iranische Entsprechung[11] eines unbezeugten Kompositums ved. **manas-ví-* mit dem Wurzelnomen der Wurzel *vī-* 'sein Augenmerk richten auf, sich jmdm./etw. zuwenden, trachten nach usw.'[12] als Hinterglied. Da ved. *mánas-* neben der geläufigeren auch gar nicht so selten die – wie das formal identische Gegenstück griech. *μένος* ausweist – ursprünglichere[13] Bedeutung 'unwiderstehlicher Drang, zügelloser Trieb, Verlangen, Leidenschaft' besitzt, ergibt sich für dieses Kompositum in Analogie etwa zu ved. *devá-ví* 'den Göttern zugewandt' ein Bedeutungsansatz 'dem *mánas-*, dem zügellosen Trieb, der Leidenschaft zugewandt/zugetan', also 'leidenschaftlich, heißblütig, draufgängerisch, ungestüm'. So verstanden, hat es mit altpers. *manauvīš* durchaus seine Richtigkeit, im Formal-Morphologischen wie im Semantischen; als einziger 'Schönheitsfehler' wäre höchstens zu bemängeln, daß eine Junktur der beiden Kompositionselemente ar. **vī-* + **mánas-* (als direktes oder indirektes Objekt) bislang nicht aufgezeigt werden kann.

Anmerkungen

[1] Vgl. zu dieser Charakterisierung vornehmlich Mayrhofer 1979.

[2] Hierzu vgl. zuletzt ausführlich, auch mit detailliertem Textvergleich zwischen DNb und XPl, Mayrhofer 1978: 21–25.

[3] Darnach ergänzt ist XPI 14f. *n-i-y : m-[n-u-vi-i-š : a]-h-mi-i-y*
= naiy ma[nauvīš a]hmiy
mit der 'Variante' *ahmiy* statt *amiy*.

[4] Aneinandergereiht sind hier nur die Übersetzungsvorschläge von Brandenstein-Mayrhofer 1964: 131; Hinz 1969: 57b; Hinz 1975: 233; Hinz 1976: 232.

[5] Vgl. hierzu Hinz 1969: 61b; Hinz 1975: 233 (s.v. **taižaxvanta-*), aber auch die kritischen Bemerkungen von Schmitt 1969: 57f.

[6] Zu diesem Fragenkreis vgl. ausführlich Mayrhofer 1980: 134f. – Es gibt ja durchaus einige Randgruppen von *-ī-*Stämmen, die keine Motionsfeminina darstellen.

[7] Vgl. Anmerkung 6.

[8] Vgl. weiter Kent 1945: 47a; Kent 1950: 202a; ebenso Brandenstein 1958: 119.

[9] So Wackernagel-Debrunner (1954: 919 § 733e), wo auf altpers. *manauvīš* als strittige Entsprechung verwiesen ist; zur Fragwürdigkeit dieser Gleichsetzung vgl. auch (anders als in Brandenstein 1958: 119) Mayrhofer 1963: 573f.; Brandenstein-Mayrhofer 1964: 131; Kuiper 1965: 301. – Dagegen faßt K. Hoffmann *apud* Rix 1981: 117 ved. *manasvín-* als Umbildung von **manasva-* < idg. **menes-u̯o-* und somit als Entsprechung von ital. **meneru̯o/â-*, der Grundlage des Namens der Göttin latein. *Menerva/Minerva* (vgl. ausführlich Rix 1981: 111–122).

[10] Kent (1950: 202a) schreibt lapidar: "Hz. Apl [d.h.: Herzfeld 1938] 242 otherwise, wrongly".

[11] Für das Phonologische genügt ein Verweis auf die Gleichung ved. *Sárasvatī-* = altpers. *Harauvatī-* 'Arachosien'.

[12] Vgl. die eingehende Behandlung dieser Wurzel *vī-* (Präsens *vétī*) bei Schmid 1968.

[13] Vgl. etwa Schmitt (1967: 103f. u.ö.); Nowicki (1976: 89f., 179 u.ö.).

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Further evidence in support of Brugmann's law

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In 1876, in the same number of Curtius' Studien that contains his analysis of the Proto-Indo-European nasalis sonans, Karl Brugmann printed his formulation of the sound law generally known as Brugmann's Law (367ff., 380–381): PIE **o* alternating with **e* becomes Indo-Iranian **ā* in open syllables, but not in absolute auslaut. The examples and categories of examples adduced have become classic: the full-grades of agent-nouns (e.g. Skt. *dā́tāram* 'giver' acc. sg.) vs. analogous forms in kinship terms (e.g. Skt. *pitáram* 'father' acc. sg.); *o*-grade *o*-stem nouns (e.g. Skt. *bhārá-* 'load, burden', Gk. *phóros* 'tribute') next to *e*-grade forms (e.g. Skt. *jánas-* 'type', Gk. *génos*); root nouns like Skt. *pā́dam* 'foot' acc. sg. (cf. Gk. *póda*); *o*-grade forms like Skt. *jā́nu-* 'knee' and *dā́ru-* 'wood, tree' (cf. Gk. *gónu, dóru*), next to *e*-grade specimens like *mádhu-* 'sweet' (cf. Gk. *méthu* 'wine'); perfect tense formations like *jajā́na* 'he begot' (cf. Gk. *gégone*) next to *jajántha* with its closed syllable. As Brugmann himself noted blandly some 20 years later (1897: 138–139), the suggestion had met with a mixed reception. Some accepted it; rather more registered doubts, more or less on general principles; but Hübschmann and Wackernagel had sturdily defended it, while Collitz and Schmidt had objected to it at length (see Brugmann 1897 for literature, but Hermann Collitz *BB* 2. 1878: 291–304). The basis for the rejection was ostensibly the number of forms left unaccounted-for by the new sound law (there are many, though their number shrinks dramatically if one accepts Jerzy Kuryłowicz's suggestion (1927) that laryngeals were consonants which created closed syllables in seṭ roots). Whatever the ostensible reason, the tone of the objections suggests that Collitz and Schmidt found something offensive or ridiculous about the proposed law.

Probably when Kuryłowicz 1927 appeared, the general view was that Brugmann's Law leaked quite a bit, but that it held water quite well enough for a sound law. With Kuryłowicz's wedding of the laryngeal theory to Brugmann's Law, a very large number of exceptions to the law disappeared. Thus, Skt. *sákhāy-* 'friend' easily conformed to Lat. *socius* and OE *secg* via an etymon **sokH$_2$oy-* (where the laryngeal accounts for both the short vowel reflex in Sanskrit and the aspirated consonant), and Skt. *gará-* 'swallowing' next to Gk. *borós* 'devouring' can be accounted for by a seṭ root **g^{w}erH-* 'swallow' and its *o*-grade *o*-stem **g^{w}orHós.*

Problems remained, of course. Laryngeals shed no light on exceptions to the law like Skt. *ghaná-* 'destroying', since there is no independent evidence from Sanskrit that the root *han-* had anything but *aniṭ* shape. (The never-say-die or algebraic laryngealists would not hesitate, bien entendu, to posit an otherwise unverifiable **g^{w}honHo-* on the strength of the short root vowel in Sanskrit.) Additionally, and perhaps most exasperatingly, this new in-

terpretation actually created a problem where none had existed before. This is actually typical of new discoveries, rather than a cause for distrust per se. The excitement concerned the singular of the perfect active. Brugmann and his followers held that Skt. *cakā́ra* was the regular reflex of PIE **kʷe-kʷor-e*, but were put to shifts to explain the 1st. sg. *cakā́ra*, presumably < **kʷe-kʷor-a*. Kuryłowicz (1927: 103) elegantly showed that reinterpreting the marker of the 1st sg. as **-H_2e*, as suggested by the Hittite ending *-(ḫ)ḫi*, provided the simplest of explanations for the short vowel in Sanskrit: it was in a closed syllable. The drawback to this account is that *seṭ* roots, like **ǵenH-* 'beget', go exactly like *aniṭ* ones: 1st sg. *jajā́na* 3rd sg. *jajā́na*, but the root syllable in the stem form **gé-ǵonH-* would have been closed in both persons, yielding *jajā́na* for both.

Surprisingly (I think), these facts led Kuryłowicz to a complete volte-face as of his 1956 monograph, which rejects the validity of Brugmann's Law altogether and seeks a quite different explanation for the lengthenings found in Indo-Iranian. He seems to argue that *o*-grades in e.g. Greek corresponding to long vowels in Sanskrit stem from the same 'morphological' forces in language, but that the latter does not simply reflect PIE *o*-grades. Part of the mechanism is something called an 'additive proportion', which in effect imagines that to the Indo-Iranian Sprachgefühl a *vṛddhi* was the *guṇa* of a *guṇa*.[1] The relevance of this is that derived forms are more elaborately marked than primary or non-derived ones; hence, by a somewhat involved line of reasoning, a derived form like the agentive suffix would have full grade *-tā́r-* in Sanskrit, while a non-derived formation like the kinship terms have the simple guṇa. Analogously, the morphological vitality of the words for 'tree' and 'knee', which accounts for their Gk. *-o-* and Skt. *-ā-*, stems from their relationship to zero-grades, compared to the frozen (and therefore amorphophonemic) forms like **medhu-* > Gk. *méthu* Skt. *mádhu-*. As to the perfects, his account (Kuryłowicz 1956: 337) is unexpected: originally, Indo-Iranian would have had short vowel in both first and third singular, such that one and the same form would have had a 'fonction primaire' (3rd singular) and a 'fonction secondaire' (1st singular). This was followed by a 'renouvellement formel' whereby the third singular enjoyed an 'introduction morphologique' of a long vowel, in accord with its primary function. What is unexpected about this is that, as a rule, Kuryłowicz regarded secondary/derived functions as receiving more elaborate marking than primary ones, such that if his theories had any predictive power at all, it is surely the first singular that should have been the focus of a 'renouvellement formel'. However, it has persuaded a number of astute scholars, such as Fredrik Otto Lindeman (1970: § 36, n. 23).

In any case, the unanswered questions raised by this 'morphological' theory are numerous and puzzling. Hans Hock (1974: esp. § 2.10.3) has discussed *n*-stem inflection (and much else of relevance) in the Rigveda in meticulous detail, and makes a number of observations suggesting a certain arbitrariness in the explanations offered by Kuryłowicz. It is easy to enlarge the list considerably. Kuryłowicz deals with the vṛddhi of Skt. *svásāram* 'sister' acc. sg. by saying (1956: 62) that its long vowel (and the *o*-grade in the cognate languages) is an argument in favor of considering it to be a motivated form, compared, presumably, to the underived forms in **-ser-* (unattested). If it is true that forms like **dru-* and **drew-* account for PIE **doru-* 'tree' and (independently) Skt. *dā́ru-*, one wonders why **tri-* 'three' universally shows non-derived vocalism (PIE **trey-*, Skt. *trā́y-*). Indeed, there are a good many classes of root and suffix in Proto-Indo-European and Sanskrit which are transparently 'derived/secondary' which show *e*-grade and short vowels respectively: the root-vowels of agent nouns and *s*-stems, not to mention such questions as why perfects were 'formes

fondées' but not imperfects or desideratives or denominatives. Additionally, why would a transparently derivative element like the *s*-stem affix show *e*-grade and short vowels in the oblique cases, except for **H₂ewsos-* 'dawn' (**eH₂usos-*?) with its opaque morphology but *o*-grade (> Skt. *-āsam* etc.) throughout?

There are two specific problems with the *u*-stems. We have briefly mentioned that, according to Kuryłowicz, the real explanation for why Gk. *dóru gónu* and Skt. *dā́ru jā́nu* are different from the more numerous type seen in Gk. *méthu* Skt. *mádhu-* is that the former are derived/secondary, which is proved (I gather) by the existence of zero grades **dru- *ǵnu-* etc. If the argument is valid, the attested forms of 'movable property, livestock', PIE **peḱu-*, Skt. *pásu-/paśú-*, Av. *pasu-*, Lat. *pecus*, etc., are quite unexpected, since zero grade forms are plainly attested in Skt. *kṣumánt-* and *purukṣú-* both 'wealthy' (Bloomfield 1909) and as *-fšu-* in Avestan compounds. The attested forms are accounted for much better by Brugmann's Law than by Kuryłowicz. A second case is less straightforward: Gk. *gnáthos* and *gnathmós* both 'jaw' demonstrate the existence of zero grade forms of **ǵenu-* 'jaw', and, if we assume something like the etymon of the Greek forms in Proto-Indo-Iranian (the obscurity of the *-na-* notwithstanding), we have an explanation for the aspirated consonant uniquely attested in Skt. *hánu-*: if **źnadha-* (or the like) were misinterpreted as an expression of a morphophonemic **źhnadha-*, inherited **źanu-* would be vulnerable to remaking as **źhanu-*, thus Skt. *hanu-*. (To cite a clear parallel, expected Eng. §*lean lent lent* < OE *lǽnan* was reinterpreted as *lend* etc., even in the teeth of the cognate noun *loan*). If these facts are relevant, Kuryłowicz would predict Gk. §*gónus* Skt. §*hā́nu-*. Brugmann's Law predicts *génus* and *hánu-*, which is closer to the mark.

It remains to defend Brugmann's Law (aided by the laryngeal theory) against the charges of inadequacy brought by Kuryłowicz in regard to perfects like *jajā́na* 'he begot', and the two *o*-grade types, the Proto-Indo-European iterative-causative and *o*-grade *o*-stem nouns; according to Kuryłowicz (1956), whether the latter two types are built to *seṭ* or *aniṭ* roots has no bearing on whether Sanskrit shows *-ā-* or *-a-* in the root vowel. This is not true, as we shall see; but first we will discuss the *seṭ* perfects.

There is an obvious analogical avenue for the rise of nonetymological forms like 3rd sg. *jajā́na*, namely the proportion *jagáma* : *jajána* :: *jagā́ma* : X; the difference between the original 3rd sg. forms *jagā́ma* and *jajána* was inexplicable in Sanskrit terms, and *jagā́ma* is vastly better differentiated. The only wonder is that the distinction failed to spread to *dadárśa* 'I, he saw'. Indeed, the obviousness and reasonableness of the analogical source for *jajā́na* etc. is so great, that I can only assume that it is the failure of forms like §*dadā́rśa* to appear that compromises the analogical explanation for *jajā́na*. I suggest that the salient trait of *jagā́ma* was syllable weight rather than vowel length pure and simple. In that case, a light but functionally ambiguous form like *jajána* aligned itself with the functionally unambiguous *jagáma*, hence analogical *jajā́na*; if the functionally ambiguous *dadárśa* aligned itself with 3rd sg. *jagā́ma, jajā́na*, etc., then the innovation of a formally distinct first singular would have required inventing a corresponding light form of the root. One can dream up such – dropping the semivowel, metathesizing it, resorting to samprasāraṇa (§*dadṛ́śa*, a sort of 'subtractive proportion', in the spirit of Kuryłowicz) – but the fact that none of them actually occurred carries no weight.

In Sihler 1980 I indicated that the reflexes of *o*-grade causative/frequentatives in Sanskrit, so far from providing any evidence against the validity of Brugmann's Law, as taught by

Kuryłowicz, confirm it emphatically. Of course, a proper test requires that we confine ourselves to roots (a) of certain Proto-Indo-European etymology and (b) which are definitely and unambiguously *seṭ* or *aniṭ*. Within these secure categories there are very few forms that fail to conform to the predictions of Brugmann's Law, pace Kuryłowicz. Curiously, on its face, the data show many fewer exceptions to the expected short vowel in seṭ roots than exceptions to expected vṛddhi in aniṭ roots (and in the later stages of the language, there is a steady growth of vṛddhi causatives to seṭ roots, an innovation that is hardly to be wondered at). This pattern is true also of *o*-grade *o*-stem nominals.

The following chart sorts causatives in the Rigveda which show both guṇa and vṛddhi of the root into Arnold's (1905) five chronological periods of the Rigveda (starting with the earliest, they are Archaic, Strophic, Normal, Cretic, and Popular):

				A	S	N	C	P
aniṭ:	*gam-*	'go'	*gāmaya-*	–	–	–	1	–
			gamaya-	–	–	–	–	2
	śru-	'hear'	*śrāvaya-*	2	3	1	1	–
			śravaya-	–	1	1	–	–
	sṛ-	'flow'	*sāraya-*	–	–	–	–	1
			saraya-	–	1	–	–	–
seṭ:	*jṛ-*	'waste'	*jaraya-*	–	3	1	–	1
			jāraya-	–	–	–	1	–
Totals:								
In accord with Brugmann's Law:				2	6	2	2	2
Contrary to Brugmann's Law:				–	2	1	1	2

The meagreness of the totals doesn't disguise two facts: in aggregate the expected forms are both earlier attested and more frequent than the unexpected ones.

Then, there are a few roots that show only one causative form, but that form is 'wrong' in terms of Brugmann's Law:

				A	S	N	C	P
aniṭ:	*kṣi-*	'possess'	*kṣaya-*	–	–	1	–	–
	nam-	'bow'	*namaya-*	–	1	1	–	–
	hṛ-	'take'	*haraya-*	–	–	1	–	–
seṭ:	*am-*	'damage'	*āmaya-*	–	–	–	–	2
	svas-	'blow'	*svāsaya-*	–	–	–	–	1

The data are even more meagre here; but it is typical that the wonky *seṭ* forms are later than the *aniṭ* ones. Otherwise in the Rigveda, not counting the roots from the first table that have both guṇa and vṛddhi forms, there are in all periods of the text 16 *aniṭ* roots with a total of 175 attestations, and 11 *seṭ* roots with a total of 174 attestations.[2] It is obvious that, so far from affording no support for Brugmann's Law, the most reliable Rigvedic evidence supports it much more reliably than it supports, say, the Law of Palatals.

With regard to deverbative nouns and adjectives, we are a good deal less secure about the ablaut grade of any given formation than in causatives and perfects. For instance, there are two types of derivatives in *-ana-*. Those like *bháraṇa-* n. 'burden' and *vacaná-* 'speaking, eloquent' are probably allied to the *e*-grade Germanic infinitive seen in Go. *bairan* 'to carry'.

Those like *vācana-* n. 'a causing to recite' and *srāvaṇa-* 'causing to flow' are often causative in meaning, and probably continue *o*-grades of the root like the causative verbs. But the net result is that one has no way of knowing for certain whether a given form is *e*-grade or *o*-grade, and accordingly is or isn't in accord with Brugmann's Law.

A similar uncertainty surrounds forms presumed to be *o*-grade *o*-stem nominals. Generally speaking, securely *seṭ* roots with *vṛddhi* in *a*-stems are quite rare in the Rigveda, but they do occur; contrariwise, *aniṭ* roots without *vṛddhi* are quite frequent in such formations in all periods of the text, often paralleling the forms seeming to point to *o*-grade in accord with Brugmann's Law: thus *subhága-* and *subhāgá-* both mean 'well-endowed, wealthy', and both are commonplace in all parts of the Rigveda. Even so, Kuryłowicz (1956: 329ff. and 1977: 163–179) exaggerates the inconsistencies and misrepresents the lateness of the *vṛddhi* forms. Before citing the facts relating to securely *aniṭ* vs. *seṭ* formations, I would like to make one general and one particular observation.

In Sanskrit, compounds are mostly made with the first element in what amounts to a stem-form; but not infrequently the first element is in a case form: *puṣṭim-bhará-* 'bringing prosperity', *vājam-bhará-* 'carrying off the prize'. There are two peculiarities about such formations. (1) Every single form of such a character in the Rigveda has *guṇa* of the root in the head of the compound, whether the root is *seṭ* or *aniṭ*; and (2) among *aniṭ* roots there are a total of 32 forms but they occur to only five roots, and the only securely *seṭ* root seen in such compounds is *dṛ-* 'pierce', which occurs eleven times. The latter suggests that a case-marked first element was not a realistic possibility for just any root, and the former leads one to wonder if an *o*-grade formation is reflected here at all.

Presumably the first question is: are these compounds ancient compared to the stem-form type, or are they secondary within Sanskrit? Brugmann (1906: § 53) assumed that compounds were ab origine reduced predicates, and so ab origine had the same case in the first element that they would have had in a finite predicate. He granted that the type with stem-form as first element is at least of Proto-Indo-European date, since they are so widely attested. Indeed, since case-marked forms are actually sparingly attested, to say the least, we gather that Brugmann's chief reasons are glottogonic, and an exactly contrary glottogonic argument is often relied upon to account for the stem-form of compounds, namely, that compounding itself is so ancient that it predates case-marking in syntax. Both arguments are weak and aprioristic. In the attested history of a language like English, it can be seen that compounds of the type *painstaking, thanksgiving*, and *groundskeeper*, showing a morphologically marked first element, are very recent (the oldest of these three is first seen in 1533).

One possibility is that the uniformly attested guṇa in the Sanskrit forms continues an *e*-grade, and that they are late. This seems daring (if not incoherent), but the reasoning goes as follows: *e*-grade derivatives with the verbal element governing the following form are attested in Gk. *pheré-nīkos* 'carrying off victory', a type seen mainly in names and corresponding probably to Skt. *trasá-dasyu-* 'name of a prince' ('tremble-foe' or the like). Now, in English the compounds that most closely approach the force of syntagms are the relatively new formations in which the verb comes first: *pinchpenny, killjoy, cutpurse*, etc., that is, in the same order as the typical finite syntagm: *pinches pennies, kills joy*, etc. My suggestion is that Sanskrit compounds of the type *puṣṭim-bhará-* started life as representatives of the *e*-grade *trasá-dasyu-* type, the rearrangement to the typical sequence of the

Sanskrit verb and its object including the adjustment of the 'object' of the 'verb' in the compound to an appropriate case (not always accusative, parenthetically).

Second, and more particularly, I would like to exclude *bhága-* m. 'gracious lord' from my evaluation of reflexes of Brugmann's Law. The word is specifically and conspicuously associated with deities and kings in the earliest Rigveda. The simplex and its derivatives, especially *subhága-* 'well-endowed' and its derivative *sáubhaga-* n. 'prosperity' are very frequent in the Rigveda. The conventional etymology of these forms takes them from *bhaj-* 'divide, share, apportion' thus: *'apportioner' > 'generous'; the use of the word as a divine epithet is an obvious piece of word-magic. But for one thing all sorts of divine beings are characterized as *bhága-* in the Rigveda, including the goddess Uṣas and, of all things, the horses of the Aśvinau. Secondly, the semantic development proposed is inside-out: notions of 'generous' derive *from* notions of divinity and nobility, not the other way around, as in the word *generous* itself < Lat. *generōsus* 'well-born, of good family'. Again, Eng. *divine* means both 'pertaining to deities' and 'uniquely wonderful, special, gratifying', but the latter meanings come from the former, not vice-versa. Accordingly, if *bhaj-* meant 'divide' from the beginning, it is an extremely unlikely source for *bhága-* 'lord'.

In the following tabulations of suspected *o*-grade *o*-stems occurring in the Rigveda, forms of dubious semantics have been discarded, e.g. *yamā́-* du. 'twins' as if from *yam-* 'reach'. Elsewhere, forms that are too likely to be *e*-grades have been winnowed out (e.g. *sará-* 'flowing, liquid', without doubt is from *sṛ-* 'flow'; but *e*-grade *o*-stems are at hand in e.g. Lat. *serum* 'whey, watery part'). Typical of the data is the set of words from *i-* 'go': *āyá-* 'arrival' (one time); *triudāya-* 'threefold approach [to the altar]' (one time) and *udayá-* 'eruption' (one time). This would be accounted two forms in *vṛddhi* for a count of two, and one form in *guṇa* for a count of one. From *ṛ-* 'move' we have only *árā-* 'distance' (50 times), viz., one form in *vṛddhi* for a count of 50.

At this rate, for the Rigveda we have the following picture of probable *o*-grade *o*-stems:

aniṭ roots:	forms in *ā*:	62	forms in *ă*:	39
	count:	264	count:	278
seṭ roots:	forms in *ā*:	12	forms in *ă*:	53
	count:	15	count:	634

As promised, the data for *seṭ* roots show an extreme preponderance of forms (both sort and count) for the *guṇa* of root predicted by Brugmann's Law. In the case of the *aniṭ* roots, the facts are both more complex and ambiguous, ambiguous because of the ever-present possibility that an *e*-grade etymon underlies the form. Indeed, the three most frequent forms in the *seṭ* roots have a distorting effect on the statistical picture: compounds in *-bhaga-* (101 times), *kṣáya-* m. 'a dwelling' (58 times), and *bhára-* 'spoils, prize' (43 times) account for 73% of the total tally of *aniṭ* forms in *guṇa*. The etymology of *bhaj-* 'share' is unclear, but there can be no serious possibility of a laryngeal root-extension confusing the picture in the cases of *bhṛ-* 'carry' and *kṣi-* 'dwell'. However, since it has become clear that Brugmann's Law does account for the attested facts of Sanskrit in a generally straightforward way (apart from obvious leveling), I am strongly inclined to see *bhára-* and *kṣáya-* as reflexes of *e*-grade formations; the root accent hints at the same interpretation.

Overall, it seems to me that the foregoing data, when joined to the findings of Hock (1974) and the general coincidence of isolated PIE *o*-grades with Skt. *ā* as pointed out in 1876 by

Brugmann, provide stronger evidence for Brugmann's Law than that which can be amassed in Sanskrit for the Law of Palatals. (Indeed, in view of the rather low predictive power of the Law of Palatals, one can only marvel that Kuryłowicz could have overlooked the possibility of ascribing, say, the most regular reflex of that law, the palatalization of reduplicated syllables in Indo-Iranian, to a purely secondary 'morphologization' proper to a 'forme fondée'.) It is my hope that this contribution to the pertinent data will secure the validity of Brugmann's Law past reasonable question.

Notes

[1] This is not the place to discuss my profound doubts about the validity of Kuryłowicz's 'additive proportion' in both theory and fact, but I will here point out that the best case that can be made for some such structure operating in the history of the language is the growth of what might be called absolute vṛddhi as a characteristic of the first syllable of certain derived words. The general outlines of the growth of this derivational mechanism are clear, and they remind one strongly of the sorts of interactions divined by Kuryłowicz in connection with his 'additive proportion', except that it shows no tendency to confine itself to ablauting vowels or open syllables, as is seen in *sáubhaga-* n. 'prosperity' (*subhága-* 'prosperous') and *dárbhyá-* 'a descendent of Darbha'.

[2] This count is somewhat smaller than the numbers given in Sihler 1980, because I have since decided that *mad-* 'be exhilarated' and *kam-* 'love' are too doubtful in etymology to be included; and also that *vṛ-* 'cover' and *vṛ-* 'choose', which can pretty confidently be traced to aniṭ and seṭ etyma, respectively, have become too confused morphophonemically to be used as evidence.

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Greek φαλιός, Latin **balan*, Old High German *bal* (?) 'marked by a blaze': a horse fanciers' multilingual symposium

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I

Linguists and classical philologists* – the man whom we are honoring today can pride himself on being both – have long been intrigued by the more than casual interest in languages and linguistics displayed by the Byzantine historian Procopius of Caesarea who flourished under the East Roman emperor Justinian (527–65) and whose magnum opus are the eight books on the history of his master's wars.[1] In addition to the Greek of his day and to Latin which still served as the official language in all parts of the empire (so that even Theodoric the Great condescended to indicate his imperial approval by signing Latin *LEGI* with the help of a golden stencil), Procopius, born in Palestine, may have considered Syrian his native tongue; he refers to Phoenician and Armenian more than once; repeatedly and, as we shall see, with special interest, he alludes to the language of his archenemies, the Persians; and in one instance, we have reasons to believe, he brings in Gothic – in Procopius' words, the φωνὴ Γοτθικὴ λεγομένη, which he says the East and West Goths as well as the Vandals have in common.

To give just a few examples. It so happened that during the military siege of the town of Petra in Kolchis an important role was played by a substance whose native Iranian designation, *nafth* (Modern Persian *naft*, also *nift* according to F. Steingass, A Persian-English Dictionary p. 1416), appears as νάφθα in Procopius' Greek text, the very form that has been adopted by almost all languages of the world today; moreover, the author was rather specific about adding the Greek equivalent as he introduced the new and foreign term: ἀγγεῖα δὲ θείου τε καὶ ἀσφάλτου ἐμπλησάμενοι καὶ φαρμάκου ὅπερ Μῆδοι μὲν νάφθαν καλοῦσιν, Ἕλληνες δὲ Μηδείας ἔλαιον "And they had filled pots with sulphur and bitumen and the substance which the Persians call 'naphtha' and the Greeks 'Medea's oil'." (VIII, 11, 36)

In a similar 'bilingual' vein Procopius, the native of Caesarea which is *el-Kaiṣârije* today, treats the reader to the Persian adoption of a loanword from Latin: Καῖσαρ, οὕτω γὰρ τὸν Ῥωμαίων βασιλέα καλοῦσι Πέρσαι (II, 21, 9) "Caesar, for thus the Persians name the king of the Romans" – in contrast to the term βασιλεύς used for the Greek-Byzantine emperor. Incidentally, the student of Old High German is reminded of that early attempt by the author of the medieval German *Annolied*, about 1100 A. D. (ed. by M. Roediger, 1895: 121), at etymologizing the German counterpart of the same word:

duo santin si den edelin Cesarem,
dannin noch hiude kuninge heizzint keisere. (vv. 271f.)

II

In the light of these and many similar instances which have been collected and critically reviewed by E. Schwyzer (1914: 303–27) – Procopius received none-too laudatory marks from his belated professorial reader –, a third 'bilingual' passage from Procopius' Gothic War may deserve our renewed attention. It tells of that dramatic moment in the war between the East Goths and the Romans with Belisarius near the Milvian Bridge (537 A.D.), when Belisarius relinquished his army command and started fighting in the front ranks. What might have caused his undoing was his horse which, while unusually experienced in battle, stood out because "his whole body was dark grey (or brown?), except that his face from the top of his head to the nostrils was the purest white." At that point Procopius adds the linguistic remark: "Such a horse the Greeks call 'phaliós' and the barbarians 'bálan'." In the author's Greek: . . . ὃς δὴ ὅλον μὲν τὸ σῶμα φαιὸς ἦν, τὸ μέτωπον δὲ ἅπαν ἐκ κεφαλῆς μέχρι ἐς ῥῖνας λευκὸς μαλιστα. τοῦτον Ἕλληνες μὲν φαλιόν, βάρβαροι δὲ βάλαν καλοῦσι. (V, 18, 6)

F. Wrede, the first to draw our attention to this passage (1891: 58 Anm. 2), at first blush suspected a form of Germanic *wala, walha* 'foreign' in the "barbarian" word. However, he quite obviously neglected the context which required a semantic equivalent of Greek φαλιός 'marked by a blaze'. Thus it was left to E. Schröder in the same year (1891: 237ff.) to come out with a very challenging analysis of the word. He started with the Greek term φαλιός (and its partly synonymous φαλός, φαλαρός[2],) for which the meanings of 'shining, white' as well as 'dappled' and most specifically 'marked (on the forehead) by a blaze' were well attested not only by early Greek authors, such as Euripides, Aristophanes, or Callimachos and their commentators but also by ancient lexicographers like Hesychius (ed. M. Schmidt 1862: 4, 228f.): φαλαρόν· λευκόν; φαλαρός· φαλιός, φαλακρός, λευκομέτωπος, λευκός, καὶ φαλέον; φαλιόν· [λαμπρόν]; φαλ[λ]ός· λευκός, or Herodianus (ed. A. Lentz 1867: I, 123. 16): φαλιὸς ὁ λευκομέτωπος.[3]

And since the stem form of the Greek adjectives, φαλ-, presupposes a PIE base with **bh-*, reflected as **b-* in Germanic **bal-*, Schröder was inclined to see in its "barbarian" counterpart the inflected (strong or weak acc. sg.) form of a Gothic nominal derivative, such as **balan* (from **bals*). Its cognates in West Germanic are the *-k-* (<PIE *-g-*, *-ĝ-*) derivatives Old High German *belicha, -o*, Old Low German *beliko* (see *Althochdeutsches Wörterbuch* 1952–: 1, 871), Middle High German *belche*, New High German (dialectal) *Belch(e), Belchen* 'bald coot' ("with a slate grey body but a shiny-white blaze from the forehead to the root of the bill"). Even more striking a parallel is the recurrence of *Belche* as the name of Dietleib's horse in the medieval German epic *Biterolf* (see *Deutsches Heldenbuch*; 1866–71: I vv. 2275, 2687, 11973). In fact, Schröder did not hesitate to rediscover the word also in various topographical designations like the mountain name *Belchen*, probably from a bald (or snow-covered?) feature of the terrain.

No doubt, in this particular instance, the linguistically inclined historian of ancient times succeeded in lining up not only two semantic equivalents in two different languages but at the same time two etymological cognates – needless to say, without being able to rationalize them as such. Indeed, the root from which both of them derive, PIE **bhel(ə)-*, also with lengthening grade **bhēl-* : **bhōl-* and zero grade **bhl̥-*, is well represented in the Indo-European family of languages.[4]

There is Old Indic *bhāla-* 'shine; forehead' (< **bhēlo-*), going back to the same root as Old Indic *bhắti* 'shines'[5], Avest. *vohvāvant-* 'provided with good light or splendor'(?) (< **vohu-wā-vant-*?)[6]; Latin *fulica* (dialectal for **folica* < **bholikā*) 'bald coot'[7]; Alban. *balë* (< **bal-jā*) 'horse with a blaze on his forehead'[8]; Armen. *bal* 'paleness, pallor'[9]; Old Church Slavic *bělŭ* (< **bhēlo-*) 'white', Russ. *bélyi*, Pol. *biały* 'same'[10]; Lithuanian *bālas* besides the more usual *báltas* (< **bolətos*) and *bálnas* (< **bolənos*) 'white'[11], Latvian *bāls* (: *bals*) 'pale, pallid', *balts* 'white'[12], Old Prussian *ballo* 'forehead' (Elbing Vocab. 77)[13]; other Germanic formations in *-l-* with lengthening grade are most probably Old English *bǣl* n., Old Norse *bál* (< **bhēlo-*) 'fire, flame, pyre'[14]; rather controversial (Erbwort or loan?) and much in need of clarification are most of the Celtic cognates, such as Welsh *bàl* adj. 'with a blaze' or Middle Bret. *baill* 'same'[15]; also the ever-repeated hypothetical Gallic **balio-* has been viewed with some scepticism[16]. – Mention must further be made of the well-known name of Achilles' immortal horse, βαλιός (with retracted accent, Ilias Book 16, 149; 19, 400)[17] as well as of the adjective and appellative βαλιός (< **baliuós*?), whose initial stops (instead of spirants, cf. Schwyzer 1953: 1, 291) suggest a non-Greek, perhaps Phrygian, Thracian, or Illyrian loanword. As is evident from some lexicographical observations in ancient times, there was little doubt even then as to the semantic concurrence of φαλιός and βαλιός: βαλιός... ἢ φαλιός τις ὢν, ὁ λευκομέτωπος καὶ ὁ λευκός (Etymologicon Magnum 186, 27ff.) and τὸ φαλιὸν δὲ καὶ βαλιόν· λέγουσιν ἐπὶ τῶν ἐχόντων τι λευκὸν ἐν τῷ μετώπῳ (Scholia in Theocritum vetera 8, 26.)[18]

To be sure, since the initial **bh-* of the PIE root **bhel(ə)-* emerges as a spirant only in Greek and Latin but everywhere else in Europe as a stop, E. Schröder's assumption that the "barbarian" word *balan* cited by Procopius was of Gothic origin seems – in the absence of unequivocal phonological criteria – to be largely supported by circumstantial evidence; Procopius does use the expression 'barbarian' often interchangeably with 'Goth(ic)'. Still, on closer scrutiny circumstances in this instance do turn out to be rather cogent, for one additional reason: the striking blaze with which Belisar's horse happens to be marked would by itself hardly bring the great Roman soldier into mortal danger, had it not been, as Procopius tells us, for those Roman mercenaries who sympathized with the Goths and therefore had deserted to their ranks. Of course, they knew Belisar as well as the horse he was riding, so they tried to have the Goths concentrate their attack on Belisar by shouting at them to aim "at the *balan* horse"; but their *Hetzruf* only made sense if they could hope that the Goths would understand which horse (and with it, which rider) they were eager to point out to them.

III

In fact, we have reason to believe that some of those Roman soldiers were no less familiar with that "barbarian" term than the Goths and the learned historiographer who reports it. Procopius himself certainly had no monopoly on it. For during his time the word happens to occur in Latin garb and context in the heading of a poem written by the Christian author Magnus Felix Ennodius, born and raised in Arles on the Rhone river, from 513–21 Bishop of Pavia, who composed a number of – painfully unwitty – epigrams, one of them entitled *De equo badio et balane* (see Ennodi Opera. 1885: 246 Carm. No. 2, 136): 'About the Chestnut-Colored Horse with the Blaze on His Forehead' (the variant *badeo* of the

manuscript reflects Vulgar Latin). We also have knowledge, through one lonely attestation from the year 1354, of a medieval French adjective *balani* 'weißgefleckt'; it occurs in a metrical line: *on roncin bron, la teste balanie* (listed in Godefroy 1880: 1, 560). Not enough, Tiktin in his Rumänisch-Deutsche Wörterbuch (1903: 1, 146f.) carries quite an extensive article on the Rumanian adj. and subst. *bălaṅ* (*băla/ĭŭ*) with such meanings as 'blond, light'; 'white' (also of the hair of domestic quadrupeds); moreover *cal bălan* 'Schimmel' as well as *Bălan* by itself used as a proper name for oxen, cows, dogs and sheep. Small wonder that this Germanic loanword for 'marked with a blaze (on the forehead)', which during the broadening contacts between the Germanic tribes and the ancient world had evidently found its way into Vulgar Latin everywhere, from the Balkans across Italy and into southern France, was not lost on a rather sophisticated intellectual – and "front-line war correspondent" – Procopius of Caesarea (See Suchier, 1894: 186ff. and Loewe, 1906: 299ff.).

We may be surprised to find the designation of a horse with that particular marking of his hair borrowed from a Germanic dialect by speakers almost through the entire ancient world. And yet, as we go through the files of early Greek medical treatment of horse diseases, that is, as we leaf through the corpus of Greek hippiatrics and try to analyze the designations of horses by their color of hair (*Corpus Hippiatricorum Graecorum* 1927: 2, 312 No. 201.), we discover in a treatise which E. Schwyzer (1929: 66, 93–99) – perhaps somewhat too boldly – has in part dated back to the fifth century A.D., that out of a dozen names at least four are of undoubtedly Germanic provenience: ὁ φάλβας 'der Falbe', cf. Old High German *falo* (< Germanic **falwa-*) 'hellbraun, fahl, falb'; ὁ βλάγκας 'the white horse' (German 'Schimmel'), cf. Old High German *blank* 'weiß, glänzend', Old English *blanca* m. 'white horse'; ὁ γρίβας (with *-β-* = [ƀ] or [v], cf. Greek Λεβία for *Livia*, Schwyzer 1953: 1, 208) 'the grey- (or dun-)colored horse', clearly from a (non-attested) Gothic **grēws* (< *Germanic **grēwaz*, Old High German *grâ*[*w*]*o*, Old English *grǣw*), the first unequivocal phonological criterion favoring Gothic as the point of origin because in contrast to West and North Germanic only Gothic would have a long *-ē-* of very closed quality (it is frequently written *ei*, the Gothic grapheme for long *ī*).

What is left is – last but not least – ὁ βάλας · καλὸς καὶ εὔτονος 'the horse with the blaze on his forehead, beautiful and vigorous', the "barbarian" designation which we had tended to trace to Gothic from the very beginning. Thus the word βάλας, far from being restricted to Belisar's horse, must have made its way, like γρίβας and the rest, from Gothic into Greek, too. As a matter of fact, Procopius does not say that Belisar's horse "bore the name of Balan" in the barbarian idiom and that of "Phalios" in Greek but rather makes the general linguistic observation that "such a horse the Greeks designate as 'phalios' and the barbarians as 'balan'." Concerning the declensional pattern, E. Schwyzer (1929: 1, 97f.) and before him E. Schröder (1891: 238) as well as M. Schönfeld (1911: XXIVf.) rightly suggested that the frequently used Gothic form in *-an* served as a point of departure for switching the borrowed word into the Greek *-ας/-αν* declension; Latin may have gone a step further and, as in the case of "extensions" like *Attilānem, Totilānem* used by Jordanes and others, may have carried the *-n-* through the system, hence Ennodius' Lat. abl. sg. *balane* (see above) and the form with *-n-* or *-n*, and even *-ni* in Romanic.

IV

In view of the impressively dense and wide distribution of forms derived from the PIE root **bhel(ə)-* etc. over the entire realm of Indo-European and of the rather convincing case that can be made for the use of the simple stem form, Germanic **bal-*, in Gothic to designate something like 'marked by a blaze', it is surprising to find not a trace of it in North Germanic; also within West Germanic only the before-mentioned residual derivations, Old High German *belicha, -o* and Old Low German *beliko* with their *-k-* (<PIE *-g-, -ĝ-*) suffixes, testify indirectly to the one-time productivity of **bal-* within that limited subarea of West Germanic. There is, however, in addition possibly one case of direct evidence – though far from being uncontested, as we shall see –, a solitary Old High German gloss listed in Steinmeyer-Sievers 1879–1922: 1, 259, 31. It happens to be recorded in identical form as *pal* in two rather old and closely related manuscripts, namely *K*, otherwise designated as Sg 911 of the Old High German Abrogans Glossary (formerly referred to as Keronisches Glossar), from the 8th century and written in the Alemannic dialect, and *Ra*, the Reichenau manuscript of the Abrogans Glossary, otherwise designated as Cod. Carlsruhe Aug. CXI, from the 10th century and likewise in Alemannic, although both go back to a (lost) Bavarian original. Its Latin lemma is spelled *flaus* instead of **flauus* '(hell) glänzend, (weiß)leuchtend', as we have reasons to rewrite it, – not only in accordance with the Latin glosses: *thoca, flauus uel uestis* and *Flauus, genus uestis* (Corpus Glossariorum Latinorum 1894: 5, 612, 35 and 5, 599, 64 respectively) which must form the lexicological background, but also in accordance with the fact that the faulty writing of Latin *flavus* with one *u* must have been so frequent that Probus Donatius Servius in the Appendix of his *De Arte Grammatica* warns: "*flavus*, not *flaus*" (see *Grammatici latini* 1855–80: 4, 198, 5 and *Thesaurus linguae latinae* 1900: 6, 1, 887). And as to the spelling of the Old High German interpretamentum *pal*, the initial *p-* for Germanic *b-* is to be expected in Upper German manuscripts of the 8th and 10th centuries (see Braune 1975: § 136 Anm. 1).

However, the entire gloss which in Steinmeyer-Sievers loc. cit. reads synoptically across the page: *flaus: pal* (*K*) – *pal* (*Ra*) and *flaus: stigma* [from Greek στίγμα 'puncture, characteristic mark', στιγμή once for 'spot on a bird's plumage'] (from the parallel passage in *R*, the Viennese manuscript, Cod. Vindob. 162 of the so-called Samanunga) and which is preceded by an equally problematic line: *Toga: stoz* (*K*) – *stoz* (*Ra*) – *Toga: lahhan*, has remained so obscure that again and again the entire complex has been suspected of textual corruptions. Hardly worth mentioning is the notion of Schlutter (1912/13: 14, 147) through "emending" *flaus* to **fluus* or **flaus* (with a deleted *-a-*) to come up with a supposedly earlier form of New High German *Flaus* 'Überrock' which, in fact, is a relatively recent loan from Middle Low German (cf. Kluge 1967: 203). Also Schenk (1912: 46) with his "explanation" *stôz* st.f. 'Toga, Obergewand' obviously has nothing at all to contribute. Much more serious is the attempt by J. Splett, author of a very thorough "running commentary" on the Abrogans (1976: 390f.), through a combination of gloss 1,259,31 with 3,152,40 and 3,191,61 to identify *bal* with what elsewhere is recorded as Old High German *ballo* or *bal* meaning the 'ball to play with', to shift the entire section and what precedes it to the subject matter of toys. This shift, however, forces him to give the word *stôz* in the same passage the highly improbable meaning of 'ball, spinning top' – a tour de force about which Splett himself has his doubts. Needless to say, if the solution should at all lie in that direction, Old High German *pal*, i.e. *bal*, would have nothing to contribute to the topic of this paper.

However, one more possibility must be touched on briefly in closing: as is evident from a very short article in Du Cange (1840–50: 3, 320), as early as the 17th century the word *flavus* was "ingeniously" restored to *clavus*, meaning 'a strip (or patch) of cloth (of a different color) on the Roman tunic' (see *Thesaurus linguae latinae* 1900–: 3, 1330; *Corpus glossariorum latinorum: Thesaurus glossarum emendatarum* 1899–1901: 6, 456; 7, 353; Splett 1976: 390). This emendation seems eminently plausible because in a dictionary of "synonyms" ("Sachgruppen" would be a more adequate term), such as the Abrogans, Latin *clavus* after a fashion goes well with *toga* and the obviously "sartorial" context of the whole gloss – the illustrations cited in *Thesaurus linguae latinae* loc. cit. again and again combine *clavus* with *toga, vestis*, and *tunica*. The emendation also would eliminate the adjectival masculine ending *-us* which clashes with the gender of every noun in the passage. It might even suggest a possible rationale for the occurrence of Old High German *stôz* there because the very first of the more technical senses of the word *Stoß* listed in *Deutsches Wörterbuch* 1854–1954: 10, 3, 473 is 'Zeugstreifen' ("in der älteren Zeit oft kostümlich besonders ausgestaltet"), i.e. 'patch, piece', German 'Flecken, Fleck(en)'; or in Upper German dialect dictionaries like the *Schweizerisches Idioticon* 1881–: 11, 1585ff. 'Saumbesatz'; see also Adelung 1774–80: 4, 789. Strangely enough, the basic meaning of Latin *clavus*, too, seems to refer to that part of a (woven) fabric where it borders on ('anstößt') and contrasts with another (cf. Pauly-Wissowa 1894–1963: 4, 4ff.)

However, what about Old High German *pal/bal*? The answer will largely depend on how precise and reliable our knowledge will eventually be of the relative chronology of glossatorial work on the Abrogans; also in this regard Splett holds out hope for further substantive progress. Yet, in order not to prejudice the case, it may be wise under present circumstances to assume that the glossator who inserted the Old High German translation *pal/bal* in the sense of '(hell)glänzend, (weiß)leuchtend' already had the Latin color adjective *flavus* before him, no longer **clavus*, for which *pal/bal* as an Old High German interpretamentum would make no sense at all. Thus we are inclined – not without resignation – to accept the solution also presented by the *Althochdeutsches Wörterbuch* (1952–: 1, 786), a solution which for the time being is based on the actual juxtaposition of the adjectives **flavus* and **bal*, that is, *flaus* and *pal*, and which allows us to point to an exact Old High German counterpart of the Gothic word shouted by Roman mercenaries at the Goths to bring down Belisar, their own former commander, and reported in a Greek narrative, almost 1500 years ago, by the Byzantine historian, Procopius of Caesarea.

Notes

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1 Procopii Caesarensis Opera Omnia (rec. J. Haury) I–III (Leipzig 1905–13). All references are to this edition. – For an English translation see H.B. Dewing. I–VII (Loeb Classical Library) London, New York, Cambridge, Mass. 1914–40.

2 See Frisk 1960–72: 2, 988f.; Boisacq 1950: 1013f.; Schulze 1892: 463f.

3 See also F. Jeschonnek, De nominibus quae Graeci pecudibus domesticis indiderunt. (Diss.) Regensburg, 1885.

[4] See Walde-Pokorny 1927–32: 2, 175f.; Pokorny 1959–69: 118ff.; Persson 1891: 20. 109; Persson 1910–12: 28f.; 117f., Anm. 2; 569 Anm. 1; Lidén 1897: 76f. and fn. 5.
[5] See Mayrhofer 1953–80: 2, 493; 496f. (doubtful).
[6] See Bartholomae 1904: 1435.
[7] See Walde-Hofmann 1938–56: 1, 559f.
[8] See Meyer 1891: 24 (cf. also Sitzungsber. d. Akad. d. Wiss. in Wien 130 [1897], 69) and Jokl 1915: 35.
[9] See Scheftelowitz 1905: 37.
[10] See Trautmann 1923: 29ff.; Sadnik-Aitzetmüller 1955: 10; Sadnik-Aitzetmüller 1963–: No. 129; Berneker 1908–13: 1, 55; Vasmer 1950–70: 1, 73.
[11] See Fraenkel 1955–65: 1, 32.
[12] See Mühlenbach-Endzelin 1923–32: 1, 272.
[13] See Trautmann 1910: 310.
[14] See Falk und Torp 1909: 267; Holthausen 1963: 15; Jóhannesson 1953: 625f.; Vries 1962: 23; Holthausen 1948: 10, Feist 1939: 77f.
[15] See Dictionary of Welsh 1950–: 250; Loth 1899: 344f.; Loth 1900: 396f.; Thomas 1900: 452; Pedersen 1925: 91ff.; Jackson 1967: § 1174.
[16] See Stokes 1894: 164; Holder 1896–1913: 1, 336, 370ff., 386f.; Dottin 1920: 230; Meyer-Lübke 1935: No. 906; Wartburg 1928: 202 (badius), 217 (balla); modified by Bertoldi 1931: 291 fn. 2; Whatmough 1949: 289.
[17] See Lexikon des frühgriechischen Epos 1951–: 2, 23 ("wahrscheinlich 'Schecke'").
[18] See Solmsen 1897: 75 (favoring Phrygian origin of βαλιός; Detschew 1957: 41 (listing also Βάλας, Βάλης, *Balius* used as proper names). Cf. also Krahe 1955: 1, 115; Brandenstein 1950–52: 76; Pokorny 1959–69: 118ff.

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Further evidence for diachronic selection: Ved. *rā̆ṣṭi*, Lat. *regit* etc.

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In two articles recently published (1981, 1982) I tried to show that in various Indo-European languages or even in dialects of a single language we often can find a type of related, but not wholly corresponding units the differences among which must be explained by generalization of one or another variant resulting from invariants in prehistoric (predialectal) paradigms, sometimes in such of the parent language itself. If this view is correct, the real existence of proto-languages like Common Aryan, Common Greek and ultimately Proto-Indo-European somehow must be acknowledged since it is presupposed by the inevitable assumption of earlier basic linguistic systems with certain characteristic grammatical subsystems (inflectional categories). There are numerous clues for such phenomena in the historical Indo-European languages unified into either direction from former alternants (invariants or allomorphs) in their proto-language. They are often well known as, for instance, the different lexicalizations of stem-alternants in neuter nouns of an old heteroclitic inflection. In the Aryan subgroup of Indo-European one should remember, in this framework, the diverging root-initial consonants in Vedic present and aorist forms of *gam* 'to go' on the one hand and in their Old Iranian counterparts on the other. The Vedic verb has *g-* throughout its synchronic paradigm (pres. ind. *gácchati*, aor.ind.-inj. [*á*]*gan*, subj. *gámat*, opt. *gamyā̆t*, causative *gā̆mayati*), whereas the equivalent Avestan one offers corresponding forms beginning with *j-* (pres.ind. *jasaiti*, aor.ind.-inj. [*uz*]*jən* Y. 46.12, subj. *jimaṯ*, opt. *jamiiā̆ṯ*, OP *ājamiyā* DPd 19, causative *jāmaiieiti*). This hardly can be understood otherwise than owing to respective selection and generalization of different variants stemming from a Common Aryan paradigm wherein a phonologically conditioned alternation between forms with plosive **g-* and others with palatalized **ǰ-* must have existed: e.g. pres. ind. **gasćati* < **g^{w}m̥sk̂eti*, aor.opt. **gamyā̆t* < **g^{w}m̥i̯eə$_1$t*, causative **gāmayati* < **g^{w}omei̯eti* vs. aor.ind.-inj. **(a)ǰan* < **(e)g^{w}emt*, subj. **ǰamat(i)* < **g^{w}emet(i)*.

Many linguistic data – familiar to Indo-Europeanists – occurring with similar slight variations in several Indo-European languages may be interpreted thus. I do not aim at going on to enumerate examples of this kind here in order to corroborate the indicated view in general. But in return it is perhaps worth calling attention to the possibility that sometimes the principle of diachronic selection suggested here can be helpful to solve hitherto remaining problems of comparative findings in detail. The discrepancy in vowel-quantity between Ved. *rā̆ṣṭi*, *rājati* 'reigns, rules' and Lat. *regit* 'directs, rules' besides further cognates with short root-vowel in some other Indo-European languages may serve as a specimen for such an interpretation.

The Sanskrit verb is attested with forms of the athematic root-present two times in the Ṛgveda. In the first passage *rā́ṣṭi* because of its syntagmatic lexical relation to *prá . . . tirate* belongs evidently to the inherited root **(ə)reĝ-* meaning something like 'to stretch, direct, rule': RV. I 104,4 *prá pū́rvābhis tirate rā́ṣṭi śū́raḥ* 'in former (times?) the (Aryan) hero pushed forward and ruled'. The meaning of 3rd pres.inj. *rāṭ* in RV. VI 12,5 seems to be less clear. Geldner (1951:104) translated 'eilt' with question-mark added in brackets and thought (in the footnote) of a mutilated form belonging to *vraj* 'to go, wander'. Yet I presume that the lexical sense of *rāṭ* here is partly comparable to that of *rā́ṣṭi* in RV. I 104,4. In the hymn VI 12, devoted to Agni, within the first three pādas of the fifth triṣṭubh-stanza the celebrated brightness (*panayanti bhā́so*) of the god is mentioned, when he, starting like a quick runner, goes lopping through the country; then we read in the fourth pāda: *ṛṇó ná tāyúr áti dhánvā rāṭ* 'like an indebted robber he *rāṭ* all over waste landscapes'. For *rāṭ* a translation 's'étend' (= 'extends') was proposed by Renou (1964b:133), who moreover referred to a potential connection between verbal 3rd sg.inj. *rāṭ* VI 12,5 and the nominal syntagma *rā́ḷ agnís* 'ruler Agni' in the beginning of the same hymn (RV. VI 12,1). In any case not only RV. I 104,4 with *rā́ṣṭi*, but also RV. VI 12,5 with *rāṭ* – this time in a metaphor of spreading fire – could allude to the Aryans' conquest pushing on, whereby they became 'rulers'. The meanings of 'expansion, extent, stretch, direction' can be compatible, for pertinent implications of sacral kingship see Gonda (1956:passim), Schlerath (1960:105s.); the objections raised by Sihler (1977) are not cogent and coupled with uneven new suggestions, see below. In the Ṛgveda there is for the rest no necessity to distinguish a verbal root *rāj* 2 'herrschen, gebieten' with thematic present-stem *rā́ja-* (as e.g. RV. I 188,1, equally said about Agni, *sámiddho adyá rājasi devó . . . sahasrajit* 'inflamed thou rule today, being a god, . . . , thou, having been conqueror thousand times') and a root *rāj* 1 'glänzen, strahlen, hervorleuchten, sich auszeichnen' comprising the two attestations of an athematic root-present-stem *rāj-* and likewise numerous examples of a thematicized stem *rāja-* (homonymous to *rāja-* 2), as is done by Grassmann (1955:1156s.) and others (e.g. Wackernagel-Debrunner 1954:3). An original distinction between *rāj* 1 'strahlen' and *rāj* 2 'herrschen' in the Ṛgveda has seriously been doubted by scholars like Oldenberg, *Noten* I 81; Geldner in his Rig-Veda-translation, passim; Renou (1964a:12; Kratylos 10:102 (1965)); Mayrhofer (1976:56). An intimate link between 'Königtum' and 'Glanz' in the Veda was emphasized by Schlerath (1960:105). A typologically parallel compatibility of the meanings 'bright' and 'ruler, king' was pointed out by Eilers (1967:94 with n. 4), in Accadian "*šarrum* 'König' ← 'glänzend'".

If we thus can uphold in general the link between Ved. *rā́ṣṭi, rā́jati* 'reigns, rules, beams' (RV.), the root-noun *rāṭ* (*rāj-*) 'king' and some verbs outside the Vedic language like Av. *°rāzaiti* (see below), Lat. *regere* etc. (Pokorny, 1959:854–857), there remain nevertheless some problems to be discussed. At first the root-inflection of Ved. *rā́ṣṭi* seems to be old, having been preserved within the Ṛgveda only in two relic-forms. A transition into the more productive thematic inflection *rā́jati* must have taken place rather early, as is indicated by Av. *°rāzaiti* (see below). But how can we understand the long vowel of *rā́ṣṭi* to have arisen from a root **(ə)reĝ-* with short *-ĕ-*? Sihler's (1977:233–236) attempt to derive the long vowel of Ved. *rā́ṣṭi, rāj-* 'king' and its Proto-Indo-European predecessor from the laryngeal of a hitherto unknown root **reə$_1$ĝ-* (separated from **reĝ-*) 'be efficacious, have *mana*', which he would see elsewhere only in Ved. *ū́rj-* (f.) 'nourishment, power, strength', can scarcely be right. The idea clearly depends upon including Ved. *ū́rj-* in order not to be

ad hoc only for Ved. *rā́ṣṭi* and the related noun *rā́ṭ*, Lat. *rēx* (and derivatives), OIr. *rí* 'king'. But instead of *ūrj-* we should expect § *īrj-* from a zero grade **ṛə₁ĝ-* (without *u̯-* !) in word-initial position (see Wackernagel 1896:28), so that this combination must be abandoned (other etymological suggestions for *ūrj-* are adduced by Wackernagel 1896:261; Mayrhofer 1976:652). Now, are there other possibilities for explaining the *-ā-* in the Aryan verb? The length of the root-vowel in the repeatedly attested Avestan iterative-stem *rāzaiia-* (*rāzaiieiṇti* Yt. 14.56 etc.) 'to direct, stretch, arrange' is worthless here since it may be due to Brugmann's law, for which Goth. *ufrakjan* 'to stretch out' with short *-a-* from **-ŏ-* and its Germanic congeners like OE *reccan*, OHG *recchen* etc. are possibly comparable. More interesting is the long vowel in the verbal adjective Av. *rāšta-* (besides *raštəm* Yt. 14.43) 'directed, arranged', OP. *rāsta-* 'straight', Turfan Pahl. *rāšt* 'true': it must be analogical, presumably borrowed from a present **rāź(a)-* corresponding to Ved. *rā́j(a)-*[1]. Old Iranian **rāžati* is attested in Av. *vīrāzaiti* 'rules' exactly squaring with Ved. *ví-rājati* 'reigns' RV. X 189,3 etc.: Yt. 14.47 *vərəθraγnəm . . . yō vīrāzaiti aṇtarə rāšta rasmana* 'Vərəθraγna . . . , who rules between the two lines of battle ranged'[2]. Consequently we have to reckon with an Indo-Iranian present-stem **rāź(a)-*. Its long root-vowel must be accounted for.

Generally it is held that Ved. *rā́ṣṭi* or its Aryan predecessor is a denominal derivative of the inherited root-noun Ved. *rā́ṭ, rāj-* (cf. Lat. *rēx*, OIr. *rí*), so that the long vowel would have its source in the nominal basis (Mayrhofer 1976:56 with further references; Joachim 1978: 145). But this explanation is doubtful for two reasons. At first, denominative verbs derived without an additional suffix from athematic noun-stems are unusual, as is rightly argued also by Sihler (1977:231s.). Rare examples as Ved. *bhiṣákti*, Av. *bišaz-* 'heals', often supposed to have been based on *bhiṣák* (*bhiṣáj-*) 'healing, physician' (Whitney 1889:387; Wackernagel-Debrunner 1954:152; Joachim 1978:144) may be primary (attested in Vedic and Avestan) and independent of the Vedic noun (compare Kuiper 1937:44–47, Mayrhofer 1963:503). Secondly, of *rā́ṣṭi*, if denominative in origin from *rāj-* 'king', only the meaning 'reigns, rules', as in RV. I 104,4, but not 'extends, pushes on', as in *rā́ṭ* RV. VI 12,5 discussed above, would be semantically conceivable. A similar objection must be raised against Sihler's (1977) results since the meaning of his suggested root **reə₁ĝ-* 'be efficacious, have *mana*' does not fit for *rā́ṭ* RV. VI 12,5 apart from the fact, that his proposal suffers from further difficulties (see above). Thus to my mind it should be maintained that the noun (Ved. *rā́ṭ*, Lat. *rēx*, OIr. *rí* 'king') and the verbs Ved. *rā́ṣṭi, rājati*, Lat. *regit* etc. all go back to the Indo-European root **(ə)reĝ-*. Then, however, we cannot help seeking for another motivation of the vowel-length in the Indo-Iranian verb and of its contrast to short *-ĕ-* in Lat. *regere* and some cognates in other Indo-European languages.

A solution of this problem can be offered in the framework of the 'proterodynamic' or 'acrostatic' root-presents discovered by Narten (1968), i.e. of an inflection with stem-gradations unknown so far. In this athematic inflection-type an accent firmly established on the first syllable ('acrostatic') was combined with an alternation between lengthened grade in forms of the strong stem and full grade in forms of the weak one (having additionally zero grade in gradable endings). Thus we have, as Narten showed, 3rd sg.pres.ind.act. Ved. *stáuti* vs. med. *stáve*, 3rd sg.pres.ind.(inj.) act. Ved. *tā́ṣṭi*, Av. *tāšt(i)* vs. 3rd pl.ind.act. Ved. *tákṣati* etc. This 'acrostatic' root-inflection must have existed already in the parent language. That is proved by remnants as 3rd pl. Ved. *ā́sate*, Gk. ἕαται (for ἧαται) 'they sit', Ved. 3rd sg. *śáye* ≈ Gk. κεῖται 'lies', all being 'Media tantum' with full grade of the root. Forms as Ved. 3rd pl.act. *stuvanti* RV. (instead of **stavati*, cf. 3rd pl. *tákṣati*) besides 3rd

sg. *stáuti* were remodelled after the more usual type bearing mobile accent (e.g. *dógdhi* vs. *duhánti*). The idea of Insler (1972:60–61) according to which the plural-active-forms of acrostatic presents would originally have had – like those of sigmatic aorists – lengthened grade, too, is purely theoretical and scarcely correct since Insler is forced to interpret the full grade of the really attested forms Ved. (pl.) *atakṣma, ataṣṭa, tákṣati, takṣur* as analogically shaped after the weak stem of the participle *tákṣat-*. Elsewhere I tried to show that the lengthened grade in all athematic inflectional categories with 'acrostatic' accent (singular indicative-injunctive active of root-presents, indicative-injunctive active of sigmatic aorists, strong cases of an acrostatic heteroclitic neuter like **i̯ék^{w}r̥/i̯ék^{w}n-* 'liver') cannot have been engendered phonetically by the immobile accent (being causal only for the primary full grade-forms), but must be due to prehistoric morphological rearrangements ('Sekundäraufstufungen') aligned after mutatis mutandis comparable paradigms (with alternations caused by mobile inflectional accent) in order to introduce stem-gradations secondarily to acrostatic paradigms that previously had only forms with full grade-stems (Strunk 1985).

Whatever may be true in this respect, the acrostatic inflection-type itself meanwhile seems to be well founded. It was vindicated with additional arguments e.g. by Narten herself (1968:15, n.44) for the verb 'to eat' with continued and generalized lengthened grade in Lith. *ė́[d]mi, ė̃sti*, OCS *jamь̆, jasti*, OLat. *ēs, ēst* vs. Ved. *átti*, by Eichner (1973:81) for Hittite 1st sg.pret. *u̯ēkun* 'I demanded' vs. 3rd pl.pres. *u̯ekkanzi*, by Beekes (1973:92) and Schindler (1975:267) at the outset of lexicalized doublets like Gk. ἦθος vs. ἔθος, γῆρας vs. γέρας etc., by myself for an athematic prototype as the basis of Lith. *bė́gu, bė̃gti*, OCS *bě̃žǫ, bě̃žati* 'to run' on the one hand and of Gk. *φέβομαι, φέβεσθαι* 'to flee' with revealing middle voice on the other (see Strunk 1985:493–495) etc. These explanations, as far as Baltic and Slavic forms are concerned, are preferable to Winter's (1978:433), according to which Lith. *ė̃sti*, OCS *jasti*, Lith. *bė̃gti*, OCS *bě̃žati* would have got their lengthened vowels by way of Winter's tentative sound-law (Indo-European short vowels before non-aspirated voiced stops are supposed to have become long in Baltic and Slavic). For at first there are several exceptions contrary to Winter's law, the most important of which is Slav. *voda* 'water' from **u̯od-*; and secondly the expressly attested vowel-length of OLat. *ēs, ēst* (see Leumann 1977:529) continuing the same inherited verb as Lith. *ė̃sti*, OCS *jasti* cannot be justified by Winter's law suggested only for Baltic and Slavic: this means that competitive explanations starting from previously acrostatic paradigms are farther-reaching than Winter's since they comprise, beyond the borders of Baltic and Slavic, comparable phenomena in further Indo-European languages as Latin (Greek, Hittite, Indo-Iranian etc.).

With regard to such other more or less ensured occurrences of Indo-European acrostatic root-presents the relic-forms Ved. *rā́ṣṭi, rāṭ* (RV.) may be attributed to this type of inflection, too. They probably continue inherited forms of the 3rd sg.pres.ind. and inj.act. **(ə)ré̯g̑-ti, *(ə)ré̯g̑-t* belonging to the normal strong stem and alternating with forms of the weak stem in the root-paradigm of **(ə)reg̑-* like 3rd pl. **(ə)ré̯g̑-n̥ti*, ptc. **(ə)ré̯g̑-n̥t-*, 3rd sg. subj. **(ə)ré̯g̑-e-ti*; for full grade of the root in subj.act. of acrostatic inflections compare Tichy (1976:79) and in particular the sigmatic aorists disclosing a characteristic opposition within the Ved. type ind. *naiṣ-* vs. subj. *neṣa-* etc. On the other hand the weak-grade-stem of the primitive paradigm with full grade of the root seems to have been generalized in some related verbs of other Indo-European languages. The most representative example of them is Lat. *regere, rego, regit* etc. 'to direct, rule' with secondary thematic vowel compar-

able to that of Myc. *e-u-ke-to*, Gk. *εὔχεται*, grown up from an old acrostatic root-present visible in Ved. 3rd pl. *óhate*, ptc. *óhāna-*, Av. 3rd. sg. *aoxte*, 3rd pl. *aojaite*, which is adduced by Narten (1968:10–12).

Probably many subsequent formations of this sort with thematic vowel were developed by passing an intermediate stage of subjunctives like **(ə₁)éugh-e-toi, *(ə)régʰ-e-ti* etc. It should be noticed that rise of certain thematic present-stems from reassigned subjunctives, once belonging to athematic indicatives/injunctives, often has been assumed in general and in detail (thus e.g. Kuiper (1937:95–104); Risch (1981:702–709); Tichy (1976:79–80, n.21); Bammesberger (1982:340–343)). Outside Latin we find correspondent formations, which can be supposed to have been likewise derived from the weak stem (subjunctive) of the old acrostatic paradigm **(ə)régʰ-/(ə)régʰ(e)-*, in the following verbs: Goth. *rikan*, with considerably shifted meaning in 2nd sg. *rikis* translating σωρεύσεις R. 12.20 'you will heap'; MHG and NHG *rechen* 'to heap', perhaps from 'to erect, stretch (out)' (see Seebold 1970:373); Gk. *ὀρέγειν* 'to reach, stretch out' with a prothetic vowel the origin of which (*ə₃-*?) is much disputed about. OIr. *rigid* 'stretches out', generally held to contain the same stem **(ə)regʰe-* as the verbs just mentioned (see Ernout-Meillet 1959:568; Frisk 1970:413; Chantraine 1961:817), is traced back to a present-formation with suffix **-ie-/-io-* by Vendryes-Bachellery (1974:R 14). If so, the Old Irish cognate must be added to a number of other verbs in Indo-European languages that, though being equally derived from the root **(ə)reg-*, do diverge in some different and at least in part not inherited stem-formations: e.g. Ved. *ṛñj(á)-, ṛ́jya-, irajyá-* (with enigmatic initial vowel *i-*), Lith. *rę́žti, rę́žiu* (with combination of a nasal and a **-ie-/-io-* present); their word-histories need not be treated of here.

Whereas Lat. *rego* and its Greek and Germanic counterparts reflect the weak stem of the prehistoric root-present with acrostatic accent, presumably via the subjunctive-stem **(ə)régʰe/o-*, the thematic stem with radical *-ā-* of Ved. *rā́jati*, Av. *°rāzaiti* besides Ved. *rā́ṣṭi* must have developed somewhat otherwise. A striking parallel is Ved. *dā́śati* 'offers, grants, honours' besides *dāṣṭi* 'id.' RV. I 127,4, that has obviously been supposed to be an acrostatic root-present, too (Narten 1968:15, n. 43; Tichy 1976:79). If Gk. *δέκομαι* (Att. *δέχομαι*) with short *-e-* (like Lat. *rego* etc.) goes back to an Indo-European subjunctive of the primary acrostatic present, as Tichy suggested, the length of *-ā-* in Ved. *dā́śati* just as that in Ved. *rā́jati*, Av. *°rāzaiti* can have been introduced only in the Aryan subgroup. Tichy (1976:78) argued that within the synchronic layer of the Ṛgveda there still seem to be subjunctives with a stem *dā́śă-* as *dā́śad* RV. IV 2,9, where this mood-function of the stem is indicated by the neighbouring unequivocal subjunctive-forms *kṛṇávate* (in the same relative clause) and *yoṣat, varat* (in the following principal clause). There is surely no doubt that other occurrences of the verbal stem *dā́śă-* clearly function as indicative-forms. Even a new subjunctive-stem with lengthened thematic vowel *-ā-* had already been developed: *dā́śāt* RV. I 68,6; 71,6; 93,3; II 23,4. Consequently the language of the Ṛgveda apparently disposed of a subjunctive *dā́śat* (belonging to ind. *dāṣṭi*) besides a more recent type *dā́śāt* (squaring with ind. *dā́śati*). That means, the pres.ind. *dā́śati* got its finally attested shape in early Vedic or perhaps Aryan times, when the original alternation between acrostatic sg. ind.act. **dé̄kti* (> Ved. *dāṣṭi*) and subj. **dé̄keti* (cf. Gk. *δέκομαι*) was replaced by a levelled stem with generalized *-ā-* of the root as in the subjunctives *dā́śat* and *dā́śāt*. In accordance with these subjunctive-stems containing a long root-vowel the thematic present *dā́śati* was remodelled from an earlier *dáśati* (cf. Gk. *δέκομαι*), that perhaps is still hidden in 1st pl. opt.act. *dā́śema* RV. VII 3,7a, where a short root-vowel is required by the trochaic cadence

of the triṣṭubh verse. The renewal of a presumable Aryan **rāźti* > Ved. *rā́ṣṭi* (RV.) to Ved. *rā́jati*, Av. *°rāzaiti* probably covered the same way. There are also occurrences of a subjunctive with long root-vowel, early attested in Ved. 1st sg.act. *vírājāni* RV. X 159,6; 174,5 and AV. I 29,6. It cannot be decided, of course, whether this form contains a subjunctive-stem *rā́jā-* adherent to ind. *rā́ṣṭi*, or *rā́jā-* squaring with the thematic ind. *rā́jati* (compare *ásāni* besides *ásti* like *váhāni* besides *váhati*). In any case, however, the Indo-Iranian present Ved. *rā́jati*, Av. *°rāzaiti* besides **rāźti* > Ved. *rā́ṣṭi* seems to have been completed, because of its long radical *-ā-*, not before the Aryan stage in the history of language just like the subjunctive 1st sg.act. **rāźā* > Ved. *rā́jā(ni)* with generalized length of the root-vowel for its part.

To sum up: Ved. *rā́ṣṭi, rā́jati*, Av. *°rāzaiti* and subj. Ved. 1st sg. *rā́jāni* reflect or are enlarged from a PIE acrostatic present **(ə)ré́ĝti*[3], subj. **(ə)réĝeti.* In other words, the long root-vowel belonging to the strong stem (singular indicative/injunctive active) of the inherited paradigm was selected and generalized. Therefore it is not necessary to take Ved. *rā́ṣṭi* or, as did Gonda (1956:164), *rā́jati* for denominal derivatives of **rāź-*, Ved. *rāj-* 'king' with unusual word-formation; at best the meaning and the morphological development of the thematic verb Ved. *rā́jati*, Av. *°rāzaiti* in being rearranged from an Indo-Iranian subjunctive may have been favoured by a vague relation to the noun **rāź-* 'king' on the basis of an analogical alignment after a 'paronymic'[4] pair consisting in the predecessors of Ved. *páti-* 'master, lord' and *pátyate* (synchronically analysable as *páty-ate*) 'possesses, rules'. Neither we are forced to assume with Sihler (1977) a hitherto unknown and isolated root **reə₁ĝ-* especially for the root-noun **rēĝ-s* and the verb Ved. *rā́ṣṭi, rā́jati* (for Gk. ἀρήγω see below n. 5). On the other hand Lat. *regere*, Gk. ὀρέγεω[5] and Goth. *rikan* with their short root-vowel **-ĕ-* exclusively continue the weak stem of the Proto-Indo-European acrostatic paradigm, presumably via its prehistoric subjunctive. Among these primary verbs Lat. *regere* contains the inherited potential meaning 'to direct, rule' besides primordial 'to stretch'[6], that is better visible in the ppp. *rēctus*, in compounds like *ērigere, por(ri)gere, surgere* etc. and is the only preserved one in the related Greek and Germanic verbs. The secondary, i.e. previously iterative verb OE *reccan*, however, means, obviously not by chance, like Lat. *regere* not only 'to stretch, extend', but also 'to rule, direct, guide'.

Starting from a Proto-Indo-European acrostatic present ind.act.sg. **(ə)ré́ĝti*, subj. **(ə)réĝeti* the resultant Indo-Iranian primary verbs ultimately seem to be based on the first, the Latin, Greek and Germanic ones on the second of these prehistoric stem-alternants.

Notes

1 See Bartholomae (1904:110 § 209.7, n. 11); borrowing of *-ā-* in Av. *rāšta-*, OP *rāsta-* from a (not attested!) *s*-aorist, as suggested by Kent (1953:34, n. 2 to § 93) is highly improbable.

2 An Av. *rāz-* 'gehen', assumed by Bartholomae (1904:1526) exclusively for Yt. 14.47, is not required by the text. The translation given above makes good sense and the conformity between Av. *vīrāzaiti* and Ved. *ví-rājati* 'rules, reigns' should not be disregarded (K. Hoffmann, by letter).

3 Schindler (by oral communication) compares OAv. *rāzarā* (n.) 'statute, order', instr. sg. *rašnā* with alternation of lengthened and full grade in an acrostatic heteroclitic noun of the same root. – Is the generalized lengthened grade of Lat. *rēx, rēgis* including derivatives (*rēgius, rēgalis*, etc.) and of OIr. *rí*, gen. *ríg* (see Sihler 1977:226) due to early levelling of an acrostatic alternation, **ré́ĝ-/ré́ĝ-* in strong and weak cases? Whatever may have happened in this paradigm, there are parallels in other root-nouns with uniform lengthened grade of the stem: compare Lat. *ēr*, gen. *ēris*, Gk. χήρ 'hedgehog' from **ĝher-*, Gk. θήρ, θηρός, Lith. *žvėrìs* from **ĝhu̯er-* (Lat. *fērus*) etc. and see Schindler (1972:37).

[4] The term 'paronymy' has been coined by Winter (1969:43–45), who emphasized the relevance of this notion for understanding certain analogical processes.

[5] If Gk. *ὀρέγειν* comes from this root, Gk. *ἀρήγειν* 'to help', *ἀρηγών* 'helper' because of their deviating initial vowel should be left aside in spite of Schulze (1933:172 n. 3), Gonda (1956:165). This etymological starting-point of Gk. *ἀρήγω*, *ἀρηγών* is rejected by Frisk (1960:137) for semantic reasons. Sihler (1977:236) would combine Gk. *ἀρήγω*, *ἀρωγός* with **rēĝ-* 'king' etc. on the basis of his suggested root **reə₁ĝ-*.

[6] Benveniste (1969:14) assumed an original meaning 'to align' (denoting special functions of a sacral kingship as e.g. *regere fines*).

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tulyāsyaprayatnaṃ savarṇaṃ Aṣṭādhyāyī (1.1.9)

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It is generally agreed upon that Pāṇini's grammar does not undertake the phonetic analysis and description of Sanskrit speech sounds. Rather it aims at formulating maximally brief and general rules to describe the phonological/morphophonological processes of Sanskṛit. It is also agreed upon, however, that some of these rules presuppose phonetic analysis and descriptions of Sanskrit speech sounds as given by the tradition from before. One such rule is *tulyāsyaprayatnaṃ savarṇaṃ* (1.1.9), which constitutes the concept of homogeneity of speech sounds and defines it in phonetic articulatory terms. Obviously, the purpose of this rule is to give an intensional definition in terms of which the class of homogeneous speech sounds can be formed and used to describe the morpho-phonological processes of Sanskṛit with generality and economy. Briefly, the rule may be glossed as follows: let the speech sounds which are articulated at the same place in the supra-glottal region and by the same type of articulatory efforts be called homogeneous speech sounds. Thus, to construct classes of homogeneous speech sounds it is first necessary to specify the places of articulation and the types of articulatory effort.

1. Places of articulation

The supraglottal region is treated as equivalent to the whole mouth, and the mouth is held to extend from the lips at one end to just before the glottis at the other. That is, the glottis is excluded from the supra-glottal region of the vocal tract. This region is further sub-divided into six functional sections for articulatory purposes. These sections are (1) *kaṇṭhaḥ* (the pharynx); (2) *jihvāmūla* (the root of the tongue); (3) *tālu* (the palate); (4) *mūrdhā* (the highest point of the palate); (5) *dantāḥ* (the teeth); (6) *oṣṭhau* (the two lips). Some of these places of articulation are selectively combined to give rise to further articulatory positions necessary for characterizing sounds which cannot be accommodated within the sections already marked off. They are as follows: (1) *kaṇṭhatālu* (the pharynx-and-palate); (2) *kaṇṭhoṣṭham* (the pharynx-and-lips) and (3) *dantoṣṭham* (the teeth-and-lips). Besides, *nāsika* (the nose) is also listed as a place of articulation.

Traditionally, the best known and widely used description of Sanskrit sounds in terms of the places of articulation enumerated above is given below. It would become evident later that Pāṇini's rule cited in the title of this paper presupposes this system of description:

(1) *akuhavisarjanīyānāṃ kaṇṭhaḥ*, i.e. [*a; k, kh, g, gh; :* (visarga) and *h* are articulated in the pharynx.]

(2) *icuyaśānāṃ tālu*, i.e., *i; c, ch, j, jh; y* and *s* are articulated at the palate.
(3) *ṛṭuraṣānāṃ mūrdhā*, i.e., *ṛ; ṭ, ṭh, ḍ, ḍh; r* and *ṣ* are articulated at the highest point of the palate.
(4) *ḷtulasānāṃ dantāḥ*, i.e., *ḷ; t, th, d, dh; l* and *s* are articulated at the teeth.
(5) *upūpadhmānīyānāmoṣṭhau*, i.e., *u; p, ph, b, bh; : p* are articulated at the lips.
(6) *ñamaṅaṇanānāṃ nāsika*, i.e., *ñ; m; ṅ; ṇ* and *n* are articulated in the nose.
(7) *edaitoḥ kaṇṭhatālu*, i.e., *e, ai* are articulated at the pharynx-and-palate.
(8) *odautoḥ kaṇṭhoṣṭam*, i.e., *o, au* are articulated at the pharynx-and-lips.
(9) *vakārasya dantoṣṭham*, i.e., *v* is articulated at the teeth-and-lips.
(10) *jihvāmūlīyasya jihvāmūlam*, i.e., jihvāmūlam (= *: K*) is articulated at the root of the tongue.
(11) *nāsikānusvārasya*, i.e. *anusvār* (= ·) is articulated in the nose.

It is clear that this system of phonetic description and classification in terms of the place of articulation groups together speech sounds of functionally different phonological classes, viz., consonants and vowels. For instance, *a, i, u, ṛ, ḷ* are grouped together with velar, palatal, bilabial, cacuminal and dental consonants, respectively. Even phonetically distinguishable sounds are not kept apart in this system of classification by place of articulation. For instance, *r* and *ṣ* together with *ṭ*, etc. are labelled cacuminal, and their phonetic differences are not revealed. Therefore, another coordinate of distinctive articulatory efforts (*prayatnaṃ*) is used to differentiate the large classes of sounds formed along the place of articulation coordinate. Incidentally, it should be borne in mind that these coordinates and their values are specific to Sanskrit speech sounds; they are not given by any general phonetic theory.

2. Articulatory efforts

There are two types of articulatory efforts: (1) Internal (*ābhyāntar*) and (2) External (*bāhya*). Internal efforts are made in the supraglottal region (from above the glottis to the lips) and external efforts are made in the sub-glottal region of the vocal tract. Efforts refer to modes of interrelation between articulators situated in the vocal tract.

Internal efforts are further subdivided into four: (1) contact (= *spṛṣṭaṃ*) between the articulators; (2) slight contact (= *iṣatspṛṣṭaṃ*) between the articulators; (3) openness (= *vivṛtaṃ*) of the articulators; (4) closeness (= *saṃvṛtaṃ*) of the articulators.

External efforts are held to be eleven in number, (1) openness (= *vivāra*) of the vocal cords; (2) closeness (= *saṃvāra*) of the vocal cords; (3) breath (= *śvāsa*); (4) vibration (= *nāda*); (5) voiceless (= *aghoṣa*); (6) voiced (= *ghoṣa*); (7) aspirated (= *mahāprāṇa*); (8) unaspirated (= *alpaprāṇa*); (9) high pitch (= *udātta*); (10) low pitch (= *anudātta*), and (11) high-low pitch (= *svarita*).

Incidentally, the first two external efforts (viz. openness and closeness of the vocal cords) are physiological, the next two (viz. breath and vibration of the vocal cords) refer to the acoustic output corresponding to the first two physiological efforts and the last two (viz., voiceless and voiced) refer to the linguistic correlates of the physiological and acoustic efforts. That is, only the first two are articulatory efforts proper; the acoustic and linguistic correlates are their results, not efforts in their own right. If this is accepted, the number of external efforts would be reduced to seven from eleven.

However, whatever be the number of internal or external articulatory efforts, classification of sounds by articulatory efforts is necessary for differentiating the large underdifferentiated classes of Sanskṛit speech sounds formed by place of articulation criterion. For example, classification by place of articulation alone gives rise to the class of palatals including *i; c, ch, j, jh; y; ś.* This is an underdifferentiated class; it includes consonants, a vowel, as well as a semi-vowel. But by using the internal articulatory effort of contact (= *spṛṣṭa*), we can differentiate *c, ch, j, jh* from the remaining *i; y; ś.* Next, the application of the internal articulatory effort of slight contact (= *iṣatspṛṣṭa*) to this group distinguishes *y* from *i* and *ś.* *y* is said to be produced by slight contact between the articulators, while for *i* the articulators are held apart, as also for *ś.* Thus, *y* and *ś* are different from each other, because *y* is characterized by the internal articulatory effort of slight contact between the articulators; *ś*, on the other hand, is produced by no contact, slight or full, between the articulators, a condition which is necessary for the production of *i* as well. In other words, *i* and *ś* are held to be produced by one and the same type of internal articulatory effort, and since they are produced at the same place of articulation in the supra-glottal region, they fulfil the conditions for homogeneity as laid down by Pāṇini's definition of homogeneity. Similar is the case with *a* and *h, ṛ* and *ṣ, ḷ* and *s*; the three pairs meet the conditions of homogeneity stated by Pāṇini's Savarṇa-rule. But from the view point of grammatical processes it is not desirable that vowels and consonants be made homogeneous with one another. However, it is unavoidable so long as the system of phonetic description and classification of Sanskṛit sounds by place of articulation and internal articulatory efforts remains unchanged. That Pāṇini himself was faced with the problem of preventing homogeneity between *a* and *h, ṛ* and *ṣ, ḷ* and *s* is evident from his rule *nājjhalau* (1.1.10). But before concluding that Pāṇini's solution is the only possible solution under the circumstances, it would be desirable to consider at least one more solution, a phonetic solution of the problem.

3. Phonetic solution

It can be easily shown that the problematic homogeneity relation between *a* and *h, ṛ* and *ṣ, ḷ* and *s* is a consequence of limiting the number of internal articulatory efforts to only four. The addition of one more type of internal articulatory effort, i.e., slight openness (= *iṣadvivṛta*) to the set of previously given four types would altogether rule out homogeneousness between the above mentioned. Then *ś, ṣ, s* would be produced by the articulatory effort of slight openness (= *iṣadvivṛta*) between articulators as different from openness (= *vivṛta*) between articulators, which is the characteristic articulatory effort for *a, i, ṛ* and *ḷ.* Indeed, in his extensive commentary on 1.1.9 in *Mahabhāṣya*, Pātañjali has discussed this possibility of increasing the number of internal articulatory efforts to five. And it should be noted that it is not an ad hoc manoeuvre proposed to solve a nagging problem regardless of its bearing on the system of phonetic description and classification. In fact the proposed internal articulatory effort of *iṣadvivṛta* is logically implied by the widely accepted, canonical articulatory effort of *iṣatspṛṣṭa*: slight contact implies slight openness. Thus, it is obvious that the inclusion of *iṣadvivṛta* in the class of internal articulatory efforts precludes the possibility of undesirable homogeneity relation between consonants and vowels. And it is equally obvious that Pāṇini did not include *iṣadvivṛta* in the class of internal articulatory efforts. Had he done it, he would not have had to formulate the rule

nājjhalau (1.1.10). Pāṇini accepted the traditional system of phonetic description and classification in its entirety and faced the problem inherent in it.

4. Pāṇinean solution

Pāṇini solves the problem of undesirable homogeneity between *a* and *h*, *i* and *ś*, *ṛ* and *ṣ*, *ḷ* and *s* by formulating an exception to the *savarṇa*-rule 1.1.9. The exception rule is *nājjhalau* (1.1.10). It prohibits vowels and consonants from being homogeneous with one another. Accordingly, homogeneity established on articulatory phonetic grounds between *a, i, ṛ* and *ḷ* and *h, ś, ṣ*, and *s* respectively by 1.1.9 is prohibited by 1.1.10.

The simplicity and finality of the solution is truly Pāṇinean and is in accordance with the organizing principle of his grammar. The principle is that if there is incongruity between phonetic and phonological frames of description; if articulatory and linguistic classifications do not coincide, let the phonological and linguistic prevail over the phonetic. Therefore, let the sounds which turn out to be homogeneous in phonetic terms be not treated as homogeneous for grammatical purposes. This strategy allows Pāṇini to accept the articulatory phonetic classification of sounds transmitted to him by tradition, but without its undesirable consequences. Tradition is preserved, even developed, since the limitations inherent in its system of phonetic description and classification are seen for what they are and are overcome.

However, what is of greater significance here, and needs stressing, is the technique and the terms by means of which Pāṇini overcomes these limitations. *nājjhalau* (1.1.10) is an apt illustration of his technique and terms. Pāṇini uses the technique of *pratyāhāra* for forming classes of Sanskrit speech sounds which are appropriate operands of high- order grammatical rules. That is, the principle that governs and regulates the classification of sounds in Pāṇini's grammar is not articulatory phonetic but linguistic and grammatical.

The rule *nājjhalau* (1.1.10) contains two *pratyāhāras: ac* and *hal.* Actually *ac* constitutes the class of all Sanskrit vowel sounds, viz., *a, i, u, ṛ, ḷ, e, o, ai, au; hal* constitutes the class of all Sanskrit consonants and semi-vowels. That is, *ac* and *hal* are two mutually exclusive classes of sounds which have different contextual functions in the language. The technique of *pratyāhāra* enables one to form as many sub-classes of these general classes as are necessary for describing the language with economy and generality. For example, the *ac*-class can be further sub-divided into smaller classes: *ec* (= *e, o, ai, au*); *aic* (= *ai, au*); *ak* (= *a, i, u, ṛ, ḷ*); *ik* (= *i, u, ṛ, ḷ*); *uk* (= *u, ṛ, ḷ*), etc. Similarly, the *hal*-class can be sub-divided into several sub-classes of consonants like: *jhal* (= *jh, bh, gh, ḍh, dh, j, b, g, ḍ, d, kh, ph, ch, ṭh, th, c, ṭ, t, k, p, ś, ṣ, s, h*); *jaś* (= *j, b, g, ḍ, d*); *khar* (= *kh, ph, ch, ṭh, th, c, ṭ, t, k, p, ś, ṣ, s*); etc. The point is that the technique of *pratyāhāra* is extremely useful in forming classes and sub-classes of sounds which are needed as operands for grammatical rules. Another feature of these classes so formed is that they are not articulatory phonetic classes; they are extensional classes of sound segments whose only justification seems to be the fact that they help in an economical and compact description of the language with maximum generality.

This is obviously different from the technique that Pāṇini uses in defining the class of homogeneous sounds in articulatory phonetic terms. And, as has been described earlier, the definition constituting the class of homogeneous sounds is not free from serious defects;

for instance, it leads to the grouping of consonants and vowels as homogeneous. The defect is eliminated by the grouping of sounds on the basis of *pratyāhāra* technique. That is, the intensional definition of the class of homogeneous sounds sanctioned by the traditional articulatory phonetic criteria gives rise to a problem; the problem is solved, however, in terms of the extensional classes of sounds formed by the powerful technique of *pratyāhāra.* Evidently, Pāṇini does not develop the technique only to prevent the undesirable homogeneity between *a* and *h, i* and *ś, ṛ* and *ṣ, ḷ* and *s*; indeed, his grammar of Sanskrit makes only a marginal use of homogeneity principle and homogeneous classes, as constituted by 1.1.9 but the sound classes produced by *pratyāhāras* form the basis of his system of description in *Aṣṭādhyāyi.*

The preceding discussion has brought to light the difficulties that the definition of homogeneity in terms of internal articulatory efforts gives rise to, and the solution that Pāṇini's grammar offers to these difficulties. However, the application of some of the savarṇa-rules for the derivation of correct grammatical forms brings to light yet another inadequacy of the definition of homogeneity by 1.1.9.

Consider the derivation of *utthānaṃ* ('rising up'). The initial appropriate rule for the purpose is *udaḥ sthāstambhoh pūrvasya* (8.4.61). Briefly, the rule states that given the sequence *ud* + *sthā* or *ud* + *stambha* let *s* of *sthā* and *stambha* be replaced by the sound which is homogeneous with *d* of the preceding segment *ud.* However, the selection of the appropriate sound homogeneous with *d* must be conditioned by *s* of *sthā* or *stambha.* In effect then, *s* is to be replaced by a sound which is homogeneous with both *d* and *s.* Now, in respect of the place of articulation *t, th, d, dh* and *n* are all articulated at the same place in the supraglottal region where *s* is articulated. But in respect of internal articulatory efforts, they are all different from *s*; while *s* is articulated with openness between articulators, they are all articulated with contact between articulators. Therefore, none of them satisfy the condition of being homogeneous with *s.*

Nevertheless, *th* is deemed to be homogeneous with *s* and is selected to replace *s* in *ud* + *sthā* or *ud* + *stambha.* This is done apparently by making external articulatory efforts, too, criterial for homogeneity among sounds. Only by this manoeuvre are *th* and *s* held to be homogeneous: *s* by its place of articulation is dental, so is *th*; *s* by its internal articulatory effort is open, but *th* is not; however, by its external articulatory effort *s* is voiceless and aspirated, so is *th.* Therefore, *s* and *th* are considered homogeneous with each other, in spite of the fact that their internal articulatory efforts are quite different. Thus, the sequence *ud* + *sthā* by *udaḥ sthā- stambhoḥ pūrvasya* (8.4.61) is first replaced by *ut* + *ththā* by *khari ca* (8.4.55), and the output of 8.4.55 is replaced by *ut* + *thā* by *jharojhari savarṇe* (8.4.65), and it in turn is replaced by *utthā* + *lyut* by *lyut ca* (3.3.115), and next by *utthanā* by *yuvornakau* (7.1.1.) and finally by *utthānaṃ* by *atoṃs* by 7.1.24. Similarly, we can derive *uttambhanaṃ.* This derivation shows that external articulatory efforts are as essential to homogeneity among sounds as are internal articulatory efforts, although 1.1.9. does not consider them for the definition of homogeneity.

One more example, which leads to the same conclusion, is given below. Consider the derivation of *vaggharih* or *vaghariḥ* according to the rule: *jhayo ho anyatarsyām* (8.4.62). The rule states that given the consonants of *jhay* -- *pratyāhār* (excluding the voiceless aspirates and unaspirates) immediately preceding *h, h* may be optionally replaced by that which is homogeneous with the sound preceding *h*; however, the replacing sound must be condi-

tioned by *h* and be homogeneous with it also. For example, let the string be *vāc + hari* → *vāk + hari* by *choḥ kuḥ* (8.2.30) → *vāg + hari* by *jhalāṃ jaśonte* (8.2.39). Now, in this string *g* immediately precedes *h*; the sounds homogeneous with it are *k, kh, g, gh, ṅ*, but none of them is homogeneous with *h* in respect of the internal articulatory efforts, because they are all produced with contact between the articulators, while *h* is produced with the openness of articulators. Therefore, the selection of the proper replacement of *h* from among *k, kh, g, gh, ṅ* is blocked. If, however, external articulatory efforts are also considered essential to the selection of the right replacement for *h* in the string, *gh* fulfils the conditions of being homogeneous with *h*. Naturally, *gh* is selected as the replacement of *h*, since *gh* is voiced, aspirated and is produced with the closeness and vibration of the vocal cords, just as *h* is voiced, aspirated, and is produced with the closeness and vibration of the vocal cords. Only then can *vāg + hari* be replaced by *vāgghari* by *jhayo ho anyatarśyām* (8.4.62) or optionally remain as *vāghari.* By certain other *sup*-affixes rules we can derive *vāggharīḥ* or *vāgharīḥ.* The point here is simple: 1.1.9 defines the class of homogeneous sounds in terms of internal articulatory *efforts*, but the application of rules stated in terms of homogeneity leads to certain difficulties, which are removed only if external articulatory efforts are considered necessary for the purpose of defining homogeneity between sounds. That is, the definition of homogeneity by 1.1.9. is at variance with the application of homogeneity in the derivation of correct grammatical forms.

Earlier on we noted how by itself *tulyāsyaprayatnaṃ savarṇaṃ* (1.1.9) led to undesirable homogeneity between consonants and vowels, but how by *nājjhalau* (1.1.10) they were declared non-homogeneous, and thus was removed the difficulty that 1.1.9 had given rise to. Now we notice that in the derivation of certain grammatical forms like *vaggharīḥ* and *utthānaṃ* the principle of homogeneity alone is not sufficient; the homogeneity principle stated as it is in terms of internal articulatory efforts is not adequate. However, there does not seem to be any rule in *Aṣṭādhyāyi* explicitly stated to remove this specific inadequacy – one is left to an ad-hoc strategy of using external articulatory efforts also as essential to the establishment of homogeneity between sounds. Or else to use *sthāne antartamaḥ* (1.1.50) to support the inadequate definition of *savarṇa* for the purpose of deriving *utthānaṃ*, etc. In short, neither the definition of *savarṇa* nor the operation of savarṇa-rules is free from deficiencies. The reason may very well be that Pāṇini has defined *savarṇa* in phonetic terms.

Two Anatolian forms: Palaic *aškumāuwa-*, Cuneiform Luvian *wa-a-ar-ša*

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1. Palaic *aškummāuwa-* 'holy, consecrated, pure, tabu'

This form is attested four times in two texts: KUB XXXV 165 Rs. 11 and KBo XIX 152 I 14 with its duplicate KBo XIX 153 III 9, 20 (restored). They are easily accessible in edited form (Carruba 1970; cf. also Carruba 1972, A. Kammenhuber, *RHA* 64.1353; *BSL* 54.1353:18–45).

The first of these tablets (Carruba 2A) is written in the Old Hittite script; it gives lengthy Palaic cultic utterances in an Old Hittite ritual, i.e., a bilingual context. The relevant passage reads:

> *nu* ^{UZU}*šu-up-pa ti-an-zi nu me-ma-i nu-ku* ^{D}*za-par-wa$_a$-az aš-ku-ma-a-u-wa-ga wa-aq-qa-kan-ta*
> [Hittite] 'They place the consecrated meats and he speaks as follows: [Palaic] "And now, Zaparwa, these consecrated meats are *waq^{qa}kant-*."'

The Palaic prayer to the god Zaparwa continues with an enumeration of the meats which are *aškumāuwa-* and then the instruction "Give them to the goddess Kataḫzipuri!" *ne* ^{D}*ka-taḫ-zi-wu$_u$-ri pi-i-*[*š*]*a*. In this passage *aškumāuwa(ga)* is clearly the Palaic equivalent of the Hittite cultic term ^{UZU}*šuppa*. Like the latter (and its Umbrian cognate *sopa* **supa**) it is neuter plural, as proved by the concord with the participial *waq^{qa}k-ant-a* and the anaphoric pronominal *n-e*.[1]

The second example of our word is found in a prayer (*malteššar*) in another ritual (Carruba 2A), where it begins a paragraph immediately following the sacrificial 'carmen' which I have treated (1979:305–314; 1972). The sentence reads, as Carruba rightly translated, *aš-ku-um-ma-u-wa-aš ha-a-an-ta ti-i-li-la ḫa-a-ri* 'The consecrated foods (n.pl.com.) are hot (3 pl.mid.), the *tilil-* (n.pl.nt.) are hot (3 sg.mid.).' The duplicate offers the spelling variant [*aš-k*]*u-ma-a-u-wa-aš*. The gender and number agreement follow the archaic Indo-European syntactic rule. On the (inherited) verb cf. Carruba (1970:53), Oettinger (1977) and Melchert (*KZ* 97. 1984:24).

Kammenhuber (1959a:56) would render the passage as 'dem kultisch reinen (Fleisch) gegenüber plaziert er die *tilila*', taking *aškummāuwaš* as dative plural and *ḫanta* as postposition; she is followed by Szemerényi (1979:315ff). But the scriptio plena in *ha-a-an-ta* (which Szemerényi ignores) forbids identifying it with Hitt. (*menah*)*handa* or OHitt. (*man*)*handa*. The duplicate 3B offers *ḫa-an-da* twice, to be sure; but this manuscript regularly shows younger spelling habits than 3A, like omission of plene-vowels and sub-

stitution of logograms for syllabic spelling: the authority of 3A's *ḫa-a-an-ta* must be recognized.[2] That a semantically differentiated animate plural *aškummāuwaš* designating something ritually pure to eat could coexist with the inanimate *aškumāwa(-ga)* = *UZUŠuppa* 'ritually pure meat' is, pace Szemerényi, entirely possible; cf. *NINDAŠuppi* 'ritually pure bread or porridge' KBo X 34 I 5, and Hoffner (1974: 184–5 and 211).

The first breakthrough in the interpretation of this curious-looking word was made by Szemerényi in a footnote (1979:314 n. 6). Rather than seeing in *ašk-* a spelling of a cluster [sk], with Carruba (1970:39), Szemerényi (1979:316) takes the Palaic word as 'a compound (*as* + *kuma-*) expanded with the suffix *-wa-*'. The second element or 'central part' *-kuma-* (rectius *-kumma-*) he brilliantly equates with Cuneiform Luvian *kumma-* (rectius *kummai-*) 'sacré, pur, tabou', a word identified by E. Laroche and H.G. Güterbock (*Ugaritica* 3:154; cf. *OLZ.* 1959:275) and Laroche 1959 6.56–57; Laroche (*CRAI* 1974:124) further saw its connection with the family of Lycian *kumaza* 'priest, *hiereús*' and related words on the trilingual Xanthos stele discovered in 1973. Note also a Hieroglyphic Luvian *ku-ma-za-sa* Kayseri 5C sentence 16. A full discussion of the Luvian and Lycian evidence for the forms can be found in Laroche (*Fouilles de Xanthos* 6.1979:68–110).

The stem *kumma-* is the semantic equivalent of Hittite *Šuppi-* and *UZUŠuppa* 'sacré, pur, tabou; holy, consecrated (to the gods), tabu (for human beings)'; it is thus precisely the opposite of Szemerényi's 'kosher'. For the notion in Anatolian, compare Hitt. *šiuni-mi-mamu kuit Šuppi adanna natta ara n-at ŪL kuššanka edun* 'But what is sacred to my god (and) not right for me to eat, I never ate' (KUB XXX 10 obv. 10, the prayer of Kantuzilis). Now once we accept Szemerényi's analysis of our word as a compound with second member *kumma-*, the problem is reduced to identifying the first member. I propose to see in Palaic *as-* the correspondent of Cun. Luv. *a-a-aš-* and Hittite *a-i-iš (aiš)* 'mouth', Hier. Luv. *a-sa₅-za-* 'speak'.[3]

The literal sense of the compound would then be something like "mouth-tabu." Such a compound has a non-Indo-European ring to it (though recall English *footsore, heartsick, headstrong, Armstrong*); syntactically it is very similar to Hittite *pattar-palḫi-* 'wing-broad', and like the latter could have been calqued on a non-Indo-European language of the region. But the components of the compound are inherited. For the form of the compound, we may either follow Szemerényi's analysis (*as* + *kumma*) + *wa-*, with the last a secondary suffix, or we may segment *as* + *kummawa-* and assume *kummawa-* is the Palaic word for 'sacred', with a different suffix from that of Luvian *kummai-*.

Most of the etymological literature on the Anatolian family of Palaic *aš-* 'mouth' (to Lat. *ōs* etc.) is conveniently summarized in Tischler (1982 s.v. *aiš/išš-*). Lindeman (1967:1188–90) reconstructs *h₃eh₁-es*, while Szemerényi (1979b:267–83) prefers **o-es*. In my view a prototype **(h₁)eh₃-es* (like **nebh-es* > Hitt. *nepiš*), whence automatically **(h₁)oh₃es* by laryngeal coloring within morpheme boundary, yielded Common Anatolian **aëš*. This gave Hittite *aiš* (OH *a-i-iš*), either with hiatus *aïs*, or *aịs* by a glide formation rule; cf. OH *(-a)-a-i-iz-zi* < **-āeti* < **-āịeti*.[4]

The Common Luvian **e* > *a* rule produced **aäs*, which was probably contracted to *ās*, though the spelling *a-a-aš* could also render the hiatus form. The existence of an **e* > *a* rule in Palaic however, as commonly assumed, is far less certain that in Luvian. But in view of the imperative 2 and 3 singular of the verb 'to be' *a-aš, a-aš-du* (Hitt. *e-eš, e-eš-du*) we are justified in assuming a stem **aäs* in Pre-Palaic, whence attested *aš*.

The origin of the word *kumma-* is disputed. Laroche (*Actes du colloque sur la Lycie antique* 2.1980) takes both *kumma-* and *suppi-* as 'substratal', but the latter is surely inherited, cf. note 1 above. Szemerényi (*InL* 4.1978:176) has proposed connection of *kumma-* to the Avestan root noun *sū-* 'prosperité' (Kellens 1974) and the family of Vedic *śū́ra- śaviṣṭha-*, Av. *sūra- savišta-* 'powerful', Greek *kū́rios* 'authoritative', *ákūros* 'without authority'. We would have a Caland-like **kuh$_X$-mo-*, plus further suffixes; if Doric *pepāmai* 'possess' belongs here the laryngeal is h_2. This etymology would be valid only on condition that **-h$_2$m-* gave Common Anatolian *-mm-*, which is uncertain. J. Schindler suggests rather (oral communication) a derivation *kumma-* < **ku̯n-mo-* with zero grade of the **ku̯en-* of Avestan *spəṇ-ta-* 'holy', Lith. *šveñtas*, OCS *svętъ*, which is semantically and formally more attractive. We are almost certainly dealing with a Common Anatolian vocable; Hittite *kunna-* 'right; favorable' doubtless contains the same root, as H.C. Melchert points out to me.

That *kumma-* is in fact Common Anatolian is indicated above all by its geographical range as an element in place-names, which seem not to have been considered in this connection in the literature.[5]

The element *kumma-* is surely to be identified in the local names URU*Kumma*, URU*Kummaḫa* (= classical Kommagēnē), in (KUR)URU*Kummanni*, the great religious center in Kizzuwatna in the southeast (classical *Comana Cappodociae*), and also in URU*Kummiya* 'the Storm God's residence, somewhere in the mountains bordering Mesopotamia to the north' (Güterbock 1951: 78). All of these must mean something like 'Holy Land', 'Holy City'; and the Greek name of the Phrygian city of *Hierapolis* in western Anatolia suggests a continuous and conscious semantic tradition going back at least a millenium. The residences of the gods were 'holy', and a new fragment of the Song of Ullikummi provides a striking collocation: KBo XXXVI 65 IV 26–8 *nepiši-wa-kan šarā* LUGAL-*iznanni paimi nu-wa-za* URU*Kummiya*[-]*aššš-a šuppa* É*MEŠ*DINGIR*MEŠ* É[*K*]*untarrann-a daḫḫi* DINGIR*MEŠ-ma-wa-kan ne*[*(pišaz ka)*]*tta memal* GIM-*an išḫuwaḫḫi* 'I will go up to the kingdom of heaven, and I will take for myself the city of Kummiya and [ ], the holy temples and the *kuntarra-* house, but the gods I will shake down from heaven like meal.'[6] I think it quite possible that *kumm-iya/kummana* were etymologically transparent to the Hittites.

Note finally, from yet another part of Anatolia, the river *ÍD Kummišmaḫaš* and *URU Kummanešmaḫ*. Since at one time this river formed the border between Hittite and Kaška territory, it must have been north of Hattusas, probably northeast (Schuler 1965: 19, n.2). The name *Kummišmaḫa* is evidently a compound with first member 'Holy'; the second member is conceivably related either to the name of the cult city *Šamuḫa*, apparently to the northeast in the Upper Country, or to the name of the city *Šammaḫa*, to the northwest in the direction of Pāla, or to both (Garstang-Gurney 1959:31; Schuler 1965:46).

2. Cuneiform Luvian *wa-a-ar-ša*

Since Meriggi (*WZKM* 53. 1956, 204–217) and Laroche (1959:152) this word has been identified with Hittite *warša-* in the meaning 'drop'; Meriggi (*OLZ* 1962.259 n.1) further suggested the derived meaning 'teardrops, tears' for one obscure passage. The equation is however less than perfect, since the Luvian word apparently is neuter plural and the Hittite animate, and since the semantic orientation of the Hittite word toward water as a weather

manifestation, 'cloudburst' (IE *ɥers-), is absent in Luvian. The one clear context, moreover, in a Hittite-Luvian ritual favors the meaning 'water' pure and simple, and the morphological concord there points unambiguously to a neuter singular.

Laroche in his edition of the passage in question (1959:152) significantly entitles it "Conjuration de l'eau et du sel". KUB XXXV 54 III 12–16 gives the 'directions' in Hittite:

nu-šš̆an ANA GAL GIR4 *kuit wātar lāḫuwan* MUN-*ya-kan anda išḫuwan n-at-kan* É-*ri anda papparšzi ANA BEL* SISKUR.SISKUR-*ya-šš̆an papparšzi nu kiš̆š̆*[*an m*]*emai*
'The water which is poured into the asphalt bowl and the salt which is shaken in it, he sprinkles in the house, and sprinkles on the master of the ritual, and he speaks as follows:' [17–21 Luvian]

[*w*]*a-a-ar-ša-at-ta* ÍD-*ti* [*na-n*]*a-am-ma-an*
[M]UN-*ša-pa a-a-la-a-ti u-wa-a*[*(-ni-ya)-ti*] *ú-pa-am-ma-an*
[*w*]*a-a-ar-ša-at-ta zi-i-l*[*a* ÍD-*i*] *an-da*
[*n*]*a-a-wa i-ti* MUN-*ša-pa-a*[*t-ta z*]*i-la*
[*(a-a-)*]*li-i u-wa-a-ni-ya na-a-*[*wa i-t*]*i*
'The water is led from the river,
and the salt is brought from the steep rock face;
the water will go henceforth into the river no more,
and the salt will go henceforth to the steep rock face no more.'[7]

It seem obvious to me that *wa-a-ar-ša* (*-tta* is an enclitic particle) corresponds to the Hittite *wātar* 'water', just as MUN-*ša* (*-pa* is an enclitic conjunction) corresponds to the Hittite MUN 'salt'. Furthermore the broken passive participle *-amman* is clearly neuter singular; *wa-a-ar-ša* cannot be neuter plural. The solution is clear: The word for 'water' is a neuter noun *wa-a-ar*, suffixed by the so-called '-*ša* case', like MUN-*ša* and all the following neuters in the passage:

(22–3) *adduwal-za utar-ša halliš-ša parattan-za*
'evil word, impurity, uncleanliness.'

This possibility was indeed seen by Carruba in his study of the Luvian -*ša* case (n. 3 and 7 supra), but he maintained the meaning 'Tropfen', for which not surprisingly the stem was to him "nicht klar." But once we recognize the meaning as 'water' we can exactly equate Luvian *wa-a-ar* with Vedic *vā́r* nt. 'water' and the animatized Avestan *vār-* m. 'rain'. See in general Pokorny (1959:80), and Mayrhofer (1976:194). Ved. *vā́r* is attested 10 times in the Rigveda, three in the family books. Two of these three show disyllabic *vaar*, as does also one clearly archaizing or archaic hymn of book X, and perhaps one instance in the Atharvaveda (IV 7.1, cf. Whitney ad loc.). The disyllabic variant is therefore certainly older and linguistically real; we must reconstruct an old *r*-stem **ueh₁-r*. Luvian *wa-a-ar-* reflects just this preform; as in *a-a-aš-* 'mouth' the scriptio plena renders either a long vowel (by contraction) or the hiatus form *waar*. The expected zero-grade **uh₁-r-* appears in ON *ūr* 'drizzle' and in Lat. *ūrīno(r)* 'dive, *mergi in aquam* (Varro)' from *ūrīna* '*water'; the latter is animatized by the same feminine suffix as *reg-īna*.

In the Rigveda we find *vā́r* used figuratively of 'milk'; Oldenberg (1912) ad IV 5.8 *yád usríyāṇām ápa vā́r iva vrán* 'which they have uncovered like the "water" (= milk) of the dawn-cows'. Geldner ad loc. and Renou (EVP 13/3.1964:37) take *vā́r* as 'cover' or 'secret'; but the secret at issue is the hidden *cā́ru pṛ́śneḥ* 'the precious [udder (Geldner), or milk

(Renou)] of the spotted cow Pṛśni'. Compare X 12.3 *duhé yád énī divyáṃ ghṛtáṃ vā́ḥ* 'that the speckled hind give heavenly ghee (and) milk'. The cadence *ghṛtáṃ vā́ḥ* recurs at X 99.4.

This metaphor may be older still. For as I hope to show at greater length elsewhere, to Vedic *vā́r* 'water; milk' corresponds Archaic Old Irish *fír* 'milk'. The Irish word is a different one from *fír* 'true'; it is glossed as *mblicht* (*bó*) '(cow's) milk' and *finn* 'white' by the native tradition. The word occurs in a single formulaic phrase *teora ferbba fíra* 'three milch cows' (accusative plural), preserved independently in two mythological-legal traditions about dawn-cows: the beginning of the law text *Cetharslicht Athgabala* (*AL* 1.64; *CIH* 325.25, 881.4, 1897.16) and a judgment of Fachtna mac Sencha in Cormac's Glossary § 585. Archaic Irish *fír* reflects a substantivized Indo-European possessive adjective **ueh₁ -r-ó-* 'having water, aqueous', from **u̯eh₁-r̥*; it allows us to specify the laryngeal of this root. Semantically the passage is 'watery' > 'milky' > 'milk'. Here *fíra* is probably genitive singular (hypostasized to *u*-stem inflexion, like *fír* 'truth'), and Kim McCone plausibly regards *ferb fíra* as semantically equivalent to Mod. Ir. *bó bhainne* 'milch-cow' (personal communication). The extraordinary archaism which *fír* is owes its preservation to being frozen in a formulaic epithet in a mythological nexus which is itself of Indo-European antiquity.

Notes

1 On the segmentation and interpretation of *aškumāuwa-ga* 'these *aškumāuwa*' I follow the excellent suggestion of H.C. Melchert (*KZ* 97.1984:25). I therefore withdraw my earlier explanation (1982: 256) of this *-ga* as a reflex of word final h_2 of the neuter plural (Watkins 1975/6), likewise that of the *-ḫa* of Pal. *wa-su-u-ḫa*, correctly seen by Melchert to be *wasu + ha*, ibid. 31–3. On the other hand I do continue to maintain that the *-ga-* of verb forms such as *tak-ku-wa-a-ti/tak-ku-wa-ga-ti* is a reflex of h_2.

2 Compare in the preceding five lines alone:

A	B
šа-me-i-ri-iš	*šа-me-ri-iš*
ḫa-la-a-iš-ta	*ḫa-la-iš-ta*
wa-a-aš-ḫu-ul-la-ti-ya-aš	*wa-aš-ḫu-ul-la-ti-ya-aš*
ḫa-a-ap-na-aš-ta	ÍD-*an-aš-ta*
ḫa-ši-i-ra-am-pí	GÍR-*an-pat*
pa-da-a-am-ma-an	*pa-ta-am-ma-an*

3 For the Cuneiform Luvian forms see Laroche (*RHA* 76.1965:45); for the Hieroglyphic Luvian see Hawkins and Davies (*JRAS* 1975:131–133). CLuv. *a-a-aš-šа*(-) shows the *-šа* 'case', a possibility seen by O. Carruba in his valuable study of the latter (1982:1–15, esp. 5).

4 Is the *i-* of the Hittite oblique stem *iššan-* a prothetic vowel, before the original cluster of zero-grade root and suffix, in an amphikinetic neuter paradigm nom.-acc. **(h₁)eh₃-(e)s*, gen. **(h₁)h₃-s-es*?

5 Laroche (*Fouilles de Xanthos* 6.1979:109 n. 42) does rightly note that *Kummani, Comana* must mean 'la Sainte', comparing Semitic Qadesh et al. References for the local names may be found in H. Ertem (1973) and G.F. del Monte and J. Tischler (1978) *s. vv.*

6 Cf. Güterbock (1952) 48. The new fragment confirms most of the latter's restorations. Col. III 22 of the same tablet (XXXIII 106) offers] x-*aš šuppa* É.[*MEŠ* DINGIR*MEŠ* in broken context.

7 O. Carruba in his version of this passage, Serta Indogerm., Festschrift G. Neumann 47–6 (1982), translates the first phrase in the traditional way following Laroche (Les gouttes sont [. . .] ées du fleuve) as 'die Tropfen sind vom Flusse ge-. . .-t', though in Gedenkschrift Kronasser 5 (1982) he envisages another possibility. Hittite *waršaš* in any case must mean 'cloudburst, downpour' (so already Friedrich, HWb.) not 'drop', as Güterbock points out to me.
For the restoration [*nan*]*amman* 'led', which fits the space and the traces, compare *nanātti* (dupl. *lalātti* 'takes'), *nananta* DLL. Luvian *waar nana-* 'aquam ducere' paralles Hittite *watar nai-*.

For *ali-* 'steep, high, sheer' and *(u)wani-* 'rock face, block of stone, stele' (Hier. *wa-ni-za*) see "The language of the Trojans" in the Proceedings of the Bryn Mawr Symposium on the Trojan War, ed. Machteld J. Mellink.

For the provenience of salt from mountain rock faces in Anatolia cf. Güterbock's comments cited by me in "Questions linguistiques palaïtes et louvites cuneiformes", in the proceedings of the Colloque Anatolien, Paris 1985 (ed. R. Lebrun: to appear).

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Old Indic *sūnú-*, Greek *huiús* 'son'

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Szemerényi (1977: 10–12) provides a convenient overview of currently held opinions on the oldest Indo-European terms for 'son'. He sides with earlier views that OInd. *sūnú-*, Lith. *sūnùs*, OCS *synŭ* clearly presuppose an antecedent form **sūnus*. Avest. *hunu-* he takes to represent OIran. *hūnu-*; the Germanic forms with short vowel of the first syllable (Goth. *sunus*, etc.) he believes, along with Dybo, to have resulted from 'secondary shortening in unaccented position'. PIE **sūnu-s* he considers (1977: 11) 'a clear derivative of the verbal root *sū-* (: Ind. *sū-tē* "gebiert"), which originally meant "offspring" but was later restricted to the sole important offspring, the male'.

He thinks that **sūnu-s* was 'an all-IE word', and that the competing form found, e.g., in Gk. *huiús* 'son', was the result of an innovation (Szemerényi 1977: 11, 52). Without really committing himself, he appears to want to derive the Greek form from **su-yu-s* and tends to think that this reconstruction finds support in Tocharian B *soy*, A *se*, adding, however, the cautionary comment (1977: 11) 'if these do in fact derive from **suyu-*, and are not borrowed'.

Elsewhere (Winter 1985: 261–263) I have presented arguments why the Tocharian and the Greek forms should be derived from PIE **swXyu-* (with different vocalizations of the first syllable in Greek and Tocharian, respectively); as to Greek, I pointed out that it seemed impossible to assume retention of plain intervocalic **-y-* of a supposed pre-form **su-yu-*.

In view of the fact that earlier scholars had connected *u-* in Arm. *ustr* 'son' directly with Gk. *huiús* (cf., e.g., Walde – Pokorny 1927: 470 with references), it is somewhat surprising that Szemerényi fails to mention the Armenian term. Obviously, an agreement between Greek and Armenian is not unexpected (cf. Solta 1960: 462–466 with references); the ties between Greek and Armenian, on the one hand, and Tocharian, on the other, are, however, even weaker than Solta (1960: 477) is inclined to believe – none of his proposed isoglosses will bear closer scrutiny. It is interesting to note that Van Windekens, while listing (1976: 615–616) more than seventy supposed agreements between Tocharian and Greek alone (the vast majority of which though cannot be upheld once one applies strict standards), contents himself (1976: 619) with enumerating just four items which are supposed to show an agreement between Armenian and Tocharian (none of which is found in Solta 1960: 460–461, and none of which is in any way persuasive). The findings of Adams (1984: 396), in his own evaluation, do not seem to be compelling enough to claim a particular closeness between Armenian and Tocharian. There thus appears to be insufficient evidence to support the assumption that Greek, Armenian, and Tocharian owed their matching forms for 'son' to shared innovation.

Traditionally, two different approaches have been chosen to explain *-*nu*- and *-*yu*-: either they were taken to be indivisible formatives (as, e.g., by Brugmann 1906: 290, 224) or sequences *-*n-u*- and *-*y-u*- (cf. Walde – Pokorny 1927: 469). Both analyses are beset with difficulties:

A morpheme *-*yu*- is not attested outside the very word for 'son' in Greek, Armenian, and Tocharian (except that Brugmann 1906: 224 is able to adduce the adjective Gk. *prāús* 'kind'); where *-*yu*- can be isolated in forms based on verbs (agent nouns, to be sure), as in Indo-Iranian and Balto-Slavic, 'son' is formed with *-*nu*- and not with *-*yu*-. There is, thus, no sound reason to consider **sūyu*- the result of an innovative reshaping of **sūnu*-: if there was no *-*yu*- that could have been transferred, no remodeling could happen, be it in the presumed group in its entirety or in each language by itself.

The bimorphemic interpretation of *-*n-u*- and *-*y-u*-, cited above from Walde – Pokorny (1927: 469), has an antecedent in Brugmann (1906: 223) as far as *-*y-u*- is concerned: *-*y*- is equated with the formative *-*ye-/-yo*- found in OInd. *sūyate*; obviously, it is then only natural to identify *-*n*- in *-*n-u*- with the nasal affix of Avest. *hunami.*

*-*u*- suffixes are not uncommon in deverbative adjectives (cf. Brugmann 1906: 176–179), and there are indications that not only plain, but also paradigmatically modified, stems could be used in such adjectives; thus, not even the tie-up with Avest. *hunami* needs to be rejected outright. Also, the difference between adjective and noun is not necessarily crucial.

Much more troublesome, though, is another point: whenever derived from a transitive verb, *-*u*- adjectives seem to have an active meaning; thus, if PIE **sū*- meant 'beget, give birth', **sūnu*- and **sūyu*- could not have been used to denote 'birth' or 'offspring', but only something with an active meaning. If this is the case, a masculine form **sūnus* or **sūyus* would be an excellent descriptive term for 'father', but most certainly not for 'son'.

The discussion so far can be summed up as follows: There is no comparative evidence that would allow us to relate **sūnu*- and **sūyu*- as derivatives with a verb base meaning 'beget; give birth'. Furthermore, there is no support for the assumption of an innovative status of **sūyu*-: therefore, **sūnu*- and **sūyu*- should be viewed as synchronically competing terms in reconstructed Proto-Indo-European.

A few simple observations seem in order here: Whatever our interpretation of *-*nu*- vs. *-*yu*-, we have to recognize that the two must be assigned a function or meaning so close to being identical in both instances that they can be called (near-)synonymous. Furthermore, it is reasonable to assume that **sū*- as found in **sūnu*- is not different in meaning from **sū*- in **sūyu*-. **sūnu*- thus can be said to be analyzable as 'A' plus 'X', and **sūyu*- as 'A' plus 'Y', with 'X' and 'Y' for all practical purposes identical in function/meaning.

The requirement 'X' = 'Y' could be met in two ways, only one of which has so far been taken into consideration. Either, **sūnu*- and **sūyu*- could have been derived from the same base by means of different, but functionally equivalent, suffixes, or, **sū-nu*- and **sū-yu*- could be compounds with different, but meaningwise equivalent, second elements.

There seems to be reason to think that Proto-Indo-European 'root nouns' could refer to actor and action/result alike. It is then not improbable to assume that OInd. *sū*- 'begetter' was matched by a concomitant 'act (result) of begetting (giving birth)'. We may thus tentatively postulate an identification of **sū*- in **sūnu-/sūyu*- not as *'genitor', but as *'generatio'. In an informal componential semantic analysis, 'son' could clearly be identified as 'male member of the first descending generation'.

If this line of argumentation is plausible, we have to find two Indo-European roots or bases that could both be used to express the notion 'first descending' and both had the shape **nu-* and **yu-*, respectively, in the zero grade. In conventional full-grade notation, the roots would have to be **new-* and **yew-*. The twofold requirement is met by two items: the first root mentioned occurs in reconstructed **newos* 'new' (cf. Pokorny 1959: 769); **yew-* is found in OInd. *yáviṣṭa-* 'youngest' and numerous related forms (cf. Pokorny 1959: 510–511). I take **sū-nu-* and **sū-yu-* to be exocentric compounds to be paraphrased as 'one who belongs to the new (young) generation'; the relationship expressed in exocentric compounds is taken to be unspecified – as in genitival constructions; possession is viewed as merely a special case of a more general relationship.

As far as form is concerned, **-nu-* in **sū-nu-* would be the first attestation of a zero-grade form of **new-*. In spite of its wide distribution, PIE **newos* must be a fairly recent formation (with its thematic stem and no trace of ablaut resembling PIE **deywos* 'divine' beside PIE **dyew-*; cf., e.g., Pokorny 1959: 184–186). Whether **yew-* should be left as it is or rewritten as **Xyew-*, cannot immediately be decided. A decision in favor of the second alternative would permit us to propose a connection with forms such as OInd. *ā́yu-* 'life', Gk. *aieí* 'always', etc. (for a list of relevant forms see Pokorny 1959: 17): this connection seems possible, but not necessarily more than that. There is, however, a further point which may be mentioned here:

Beside **sūnu-* we find **sunu-* reflected in a number of languages (Germanic, but possibly also Iranian, though here the Old Indic parallel supports a reading **hūnu-*). Loss of length in unaccented position, as advocated by Dybo and Szemerényi, is not a well-supported explanation. Suppose now that **-yu-* in **sūyu-* was to be derived from **Xyew-* and **-nu-* from **new-*, and suppose the base to be posited for 'beget; give birth' was an aniṭ root **sew-*, the regular results of composition would be **su-nu-* and **su-Xyu-*, the former then with a regular short vowel in the first syllable, the latter with what usually (though not in Tocharian) developed into a long vowel. Suppose further that **sunu-* and **sūyu-* existed side-by-side in pre-Balto-Slavic and pre-Indo-Iranian: it would then be very easy indeed to postulate secondary lengthening of **sūnu-* under the influence of **sūyu-* with regular length.

One major issue has not been discussed by now: The order of segments in the alleged exocentric compound is the reverse of what we normally find: not modifier+head as in *longlegs*, but head+modifier. Rather than take this to invalidate the proposal made here, I would like to suggest that the compounds denoting 'son' be included among the relatively small number of compounds of the head+modifier type (Gk. *helkekhítones, helkesípeplos*) which I take to be survivors from a time when Proto-Indo-European was a language of the VX – and not, as arguable for a later period, of the XV – type. Since I presented independent reasons for this claim elsewhere (in Winter 1969: 217–218 and in a lecture given at the Historical Linguistics Conference held in 1983 at Poznań, the latter to be published shortly), I shall limit myself to this brief statement, but add to it the further comment that if the interpretations offered here are acceptable, the inherited terms for 'son' in Indo-European would belong to a very old layer of the reconstructed language, and that this claim apparently applies to both **sūnu-* and **sūyu-*.

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Inscriptionis palaeo-Osseticae apud Zelenčuk flumen repertae lectiones quaedam novae proponuntur

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Graecos, qui ipsi populorum Semiticorum quondam discipuli fuerant, multas alias nationes gentesque artem scribendi docuisse inter omnes constat; fons tamen scientiae Graecia non solum temporibus antiquis erat, sed etiam aetate recentiore, quam Byzantinam appellare solemus.

Gentes Iranicae, quae in ora septemtrionali Ponti Euxini nec non a Caucaso nivoso septemtrionem versus vivebant, bis cum hominibus, rebus, litteris Graecis commercium habuerunt. Quas Scythicas Sarmaticasque fuisse scimus gentes iam ante aetatem Christianam cum oppidis Graecis in litore Ponti Euxini conditis commercium haud minimum habebant: singulos Scythas Sarmatasque in oppidis Graecis vixisse testimonio et auctorum et inscriptionum novimus nec non res originis Graecae varias, ornamenta, vasa, supellectilem domesticam dico, in Scytharum Sarmatarumque tumulis repertas esse scimus. Quamquam illi auctores antiqui Graecorumque oppidorum inscriptiones nonnulla vocabula et nominum complures centurias nobis proferunt, tamen Scythas vel Sarmatas ipsos Graece scribere potuisse nullo testimonio cognovimus.[1]

Post multa saecula, Scytharum Sarmatarumque nomine in Ponti Euxini litore septemtrionali iam propter populorum magnam permutationem, quam gentium migrationem appellamus (saec. V, VI aet. Christianae), oblivione iam obruto, solum posteri eorum, Alanos dico, in eis regionibus habitabant; quorum Alanorum Ossetae hodierni progenies sunt. Aetate nostra Ossetae in medii Caucasi vallibus et promunturiis vivunt; attamen nomina locorum et imprimis fluminum testimonium tale dant, ut dubitare non possimus, quin Ossetae antiqui medio aevo immensam illam regionum planitiem usque ad Maeotidem et ultra Tanaim ad Danaprim usque extentam tenuerint.[2]

Qui Ossetae antiqui ab imperio Byzantino non solum religionem Christianam receperunt, sed etiam eisdem temporibus litteras Graecas didicerunt: quod gentium Iranicarum in hac parte mundi viventium cum Graecis alterum commercium erat. Tertiodecimo saeculo medio Guilelmus Rubruquius, qui eas partes terrae visitaverat, Alanos, qui Aas (Ossetae) appellabantur, Christianos esse et litteris Graecis uti enarrat (qua de re cf. Basilium Joannis filium ab Aba prognatum in opere laudato p. 268 optime disserentem). Attamen posteriore tempore litterarum Graecarum memoria interiit, ita ut incipiente aetate nostra, saeculo undevicesimo, Ossetae litteras partim Georgicas partim Rossicas accipere debuerint; occasione data etiam Arabicis litteris paululum scribebant et vicesimo saeculo, post magnum bellum primum et post magnam rerum Rossicarum permutationem, sed non infra Josephi Bes-

sarionis filii dictaturam, Ossetae etiam litteris Latinis utebantur. Attamen retro nobis ad medii aevi res litterasque properandum est.

Unicam[3] quam novimus inscriptionem palaeo-Osseticam anno 1888 D.M. f. Strukovius, vir clarissimus, societatis archaeologicae Petropolitanae sodalis praeclarus, in ripa fluminis Zelenčuk in regione quae Kuban appellatur repperit imaginemque ipsius delineavit et eam professori Petropolitano Pasicrati Millerio, Rossici imperii Academiae sodali celeberrimo de rebus Osseticis optime merito, edendam tradidit. Paene omnes sequentes editiones ab hoc exemplari Strukoviano pendent lapide quod sciam obruto. Pasicrates (Rossice Vsevolod) Millerius acerrimo ingenii sui acumine inscriptionem litteris Graecis saeculi undecimi vel duodecimi sed lingua Ossetica scriptam esse recognovit eamque interpretatus anno 1893 optime edidit.[4] Post editionem principem Millerianam editio illa, quam Basilius Joannis filius ab Aba[5] prognatus (qui Ossetice Abaity[5] Ivany fyrt Vaso, Rossice Vasilij Ivanovič Abaev appellatur), Muscoviae professor eminentissimus omniumque qui nunc florent Ossetarum decus atque delectamentum, anno 1944 et denuo 1949 publici iuris fecit,[6] maximae est auctoritatis, cum doctissimus ille vir permultas lectiones interpretationesque novas et eas Millerianis meliores protulerit.

Titulum, cuius lineamentorum imaginem lucis ope factam in figura prima, lector benevole, invenies, litteris Graecis scriptum post Millerium clarissimum Basilius Joannis filius, vir eminentissimus, optime et perlegit et interpretatus est: 1 Ἰ(ησοῦ)ς 2 Χ(ριστό)ς. 3 ὁ ἅγιος[7] 4 Ν(ι)κόλα- 5 ος. 6 Σαχη- 7 ρη φουρτ 8 Χ... 9 ...ρη φ- 10 ουρτ 11 Πακα- 12 θαρ. Πα- 13 καθα(ρ)η 14 φουρτ 15 Ανπα- 16 λ. Αν⟨α⟩πα- 17 λανη φ- 18 ουρτ 19 Λακ. α- 20 νη τζηρ- 21 θε. ΟΘΣΛ.

In inscriptionis initio Iesus Christus et Sanctus Nicolaus (qui ab antiquis Ossetis valde colebatur) invocantur. Sequuntur nomina virorum defunctorum ordine Ossetico exhibita: in lingua Ossetica imprimis patris nomen in genitivo laudatur, quod vox *furt = fyrt*, 'filius' sequitur, deinde filii nomen in nominativo. Si exempli gratia editoris clarissimi nomen perscrutamur, videmus supra eum Ivan-y fyrt Vaso = Joannis filium Basilium appellari; genitivus in *-y* desinere solet. Eodem modo viri defuncti in titulo nostro appellantur: Σαχηρη, i.e. (per itacismum aetatis Byzantinae) *Sachir-i* (gen.) φουρτ[8] (= filius) X... (nomen filii non legitur). In linea 9 habemus *-ρη*, i.e. *-r-i* (nomen patris in genitivo non legitur) φουρτ Πακαθαρ (i.e. Oss. *Bäqatär*). Sequitur in linea 12–13 Πακαθα(ρ)η[9] φουρτ Ανπαλ (i.e. Oss. *Ämbal*); 16 Αν⟨α⟩παλανη[10] (i.e. *Ämbaläⁿ-y*) φουρτ Λακ (i.e. Oss. *Läg*). Etiam sequentia vocabula Basilius Joannis filius optime interpretatus est: *ανη τζηρθε* = Oss. *ani cirtä* 'eorum monumentum'.

Quattuor ultimae litterae ΟΘϹΛ neque a Pasicrate Millerio eminentissimo neque a Basilio Joannis filio doctissimo explicatae sunt. Attamen Gregorius Theodori filius Turčaninovius Leninopolitanus, philologus celeberrimus, primam litterarum ut solis symbolum Ο = Θ, secundam ut ϑ ′ = 9, tertiam ut σ ′ = 200, quartam ut λ ′ = 30 interpretatus est; quorum symbolorum numerorumque significatio secundum doctum Turčaninovium ita intellegenda est:[11] symbolum solis Θ annos per aeram ut dicimus solarem vel per solis revolutiones computandos esse indicat; σλ ′(= 230) numerum talium solis revolutionum indicat, quarum una duodetriginta (28) annos comprehendit: itaque ducenties tricies duodetriginta faciunt sex milia annorum et quadringentos quadraginta (230 x 28 = 6440); ϑ ′(= 9) novem annos in ducentesima tricesima prima solis revolutione iam elapsos esse significat. Hoc modo annorum numerus sex milia quadringenti undequinquaginta (6449) est, quos Turčaninovius

doctus ut dicitur a creatione mundi computatos esse putat; cum primus aetatis Christianae annus quinquies millesimus quingentesimus octavus a creatione mundi sit, titulum anno aetatis Christianae uno et quadragesimo et nongentesimo (6449 – 5508 = 941) scriptum esse mathematicus noster contendit.[12]

Quam opinionem plus ingenii quam veritatis praebere neminem fallere potest: insoliti litterarum significatus (O = solis symbolum), insolentes numerorum computationes binae, immo ternae (numeri primum in duas turmas digerendi, deinde secunda turma multiplicanda, tum omnia addenda sunt), inaudita in inscriptionibus secundum solis revolutiones annorum numeratio, haec omnia faciunt, ut Turčaninovii docti opinionem veri dissimillimam esse haud sit quin ignoret.

Quae cum ita sint, post editionem Basilii Joannis filii ab Aba prognati duo loci soli in titulo restant, qui non comprehendantur: lectionem linearum octavae et nonae et ultimarum lineae unius et vicesimae litterarum significationem dico.

Quod ad primum locum incertum pertinet, me iudice ut sequitur legendus est: Quin prima octavae lineae littera X sit, non est, qui dubitet; sequitur O imperfectum (dextra circuli parte sola extante) et clarum B; littera sigma C sequitur, minor quam ceterae litterae: ad quam rem confer omicron parvulum, primam quintae et primam decimae lineae litteram; ultima deinde lineae octavae littera H, eta male scriptum esse videtur, ad cuius formam tertiam lineae nonae litteram conferre velis, quae quin etiam H sit, non est, qui dubitet.[13] Prima nonae lineae littera quae dicitur ligatura *στο* esse videtur,[14] cuius omicron imperfecte aut scriptum aut delineatum est; ligaturae lectio *στε* veri minus similis est. Sequentes litterae ΡΗΦ bene leguntur. Quae supra de nominum Osseticorum ordine diximus in mente habentes, videmus post *furt* 'filius' (lin. 7) in linea octava nomen filii in nominativo sequi debere; nec minus necesse esse, ut alteri vocabulo *furt* (lin. 9, 10) nomen patris in genitivo praecedat: quod patronymicum in *-ρη* (= -ri) cadere iam constat. Quae cum ita sint, lineas sextam usque ad duodecimam duobus modis legere possumus: aut *Σαχηρη φο[ῠ](ρτ)* Χο̨*β*, *Σηστορη φουρτ* Π*ακαθαρ* aut *Σαχηρη φο[ῠ](ρτ)* Χο̨*β*ς, Η*στορη* etc.; itaque patris Sachir appellati filius aut *Chow* aut *Chows* et Baqatari pater aut *Sistor* aut *Istor* appellabatur. Lectio sive ut ita dicam verborum delimitatio Χο̨*β*ς, Η*στορη* multo melius quam Χο̨*β*, *Σηστορη* est, quia nomen Η*στορ* optime explicatur: osseticum *stur* 'magnus' cum vocali prothetica [i-].[14a]

Quod ad ultimas unius et vicesimae lineae litteras pertinet, piam formulam Christianam prae oculis habere credimus. Litteras ΘC plane vocem Θ(*εό*)ς breviter scriptam esse fallit neminem; ultima littera ut nostra fert opinio non Λ sed litterae N imperfecta aut scriptura aut delineatio est, ipso N pro Graeca voce *νικᾶ* 'vincit' breviter scripto stante (Turyn 1972: tabula 49). Cum titulus noster Christianus sit, optime in initio invocatio Ἰ(*ησοῦ*)ς Χ(*ριστό*)ς et in fine formula ὁ Θ(*εό*)ς [*ῠ*](*ικᾶ*) 'Deus vincit' legitur, quae formula in omnibus aetatis Byzantinae scriptis frequentissime reperitur.

Itaque secundum nostram opinionem inscriptio hoc modo legenda vertendaque est:

1 Ἰ(*ησοῦς*) 2 Χ(*ριστό*)ς.
3 ὁ ἅγι(*ος*) 4 Ν(*ι*)κόλα- 5 ος.
6 Σαχη- 7 *ρη φο*[*ῠ*](*ρτ*) 8 Χο̨*β*ς.
Η- 9 *στορη φ*- 10 *ουρτ* 11 Π*ακα*- 12 *θαρ*.
Π*α*- 13 *καθα*(*ρ*)*η* 14 *φουρτ* 15 Α*νπα*- 16 λ.
Α*ν*⟨*α*⟩*πα*- 17 *λανη φ*- 18 *ουρτ* 19 Λ*ακ*.
α- 20 *νη τζηρ*- 21 *θε*.
ὁ Θ(*εό*)ς [*ῠ*](*ικᾶ*).

Versio: Iesus Christus
Sanctus Nicolaus
Sachir-ii[15] filius Chovs
Istor-ii filius Pakathar.
Pakathar-ii filius Anpal
Anpalan-ii filius Lak.
Eorum monumentum (stela).
Deus vincit.

Media inscriptionis pars hodierna lingua Ossetica reddita et litteris Latinis expressa talem formam habet:

Sachiry fyrt Chovs.
Ystury fyrt Bäqätar.
Bäqätary fyrt Ämbal.
Ämbalany fyrt Läg.
Ani cirtä.

Videmus Osseticam linguam saeculi undecimi vel duodecimi iam talem esse, ut eam hodie novimus: pristinae formae Iranicae (*furt*, Av. *puθra-*, Scrt. *putra-* 'filius', *stur*, Av. *stūra-* 'magnus, fortis') cum formis Caucasicis iam mixtae sunt; imprimis nominum propriorum syntaxis, quae in Scythicis Sarmaticisque tertii et quarti saeculi nominibus (Zgusta 1954) aliarum Indoeuropearum linguarum modum (i.e., primum nomen filii in nominativo, deinde nomen patris in genitivo) sequebatur, in inscriptione nostra iam formam hodiernam, ab origine extraneam habet.

Quae res maximi momenti est, quippe quae Osseticam linguam iam ante saeculum undecimum vel duodecimum formis modisque Caucasicis infusam esse demonstret.[16]

Notulae

1 De omnibus his rebus viri docti Max Vasmer (1923); V. I. Abaev (1949); J. Harmatta (1950–51: 261–312; L. Zgusta (1954), aliique bene egerunt. Cf. etiam notam 3.

2 De quibus rebus in Abaevi doctissimi opere supra laudato lector benevolus plus legere poterit.

3 Alias inscriptiones semiticis litteris scriptas, quae difficillimae interpretationis sunt, palaeo-Osseticas esse contendit G.Th. f. Turčaninovius (1971).

4 V. Miller 1893:110–118.

5 *Aba* (et Latine et Ossetice) nomen totius gentis progenitoris prisci est; Oss. *Abaity* nomen gentis in genitivo pluralis est: "Abaetidarum".

6 Abaev 1944; denuo typis expressum in Abaev 1949:260–270. In hac editione etiam tituli exemplar lucis ope depictum est, quod in opusculum nostrum recepimus.

7 Lectio ὁ ἅγιος non est satis clara. Pasicrates Miller, Osseticarum rerum indagator primus, οα.ς legens tertiam litteram ut Graecum τ interpretari voluit, in vocabulo οατς Osseticum *wac* 'sanctus' inesse subodorans; quae lectio valde dubia est, imprimis quia tertia littera Graecum τ esse non potest, deinde quia Sanctus Nicolaus Ossetice *wac Nikkola* appellatur: Osseticum οατς cum Graeca nominis terminatione -ος minime congruit, ut optime Basilius Joannis filius ab Aba prognatus perspicacissimus cognovit. Cuius viri doctissimi lectio ὁ ἅγιος multo melior est: tertia ligatura fortasse ut γι nimis cursim scriptum legi potest; ultima autem littera, circulus ille medius, omicron cum littera sigma ligatum et imperfecte vel scriptum vel delineatum est. Licet lectio incerta sit, nihilo minus vocabulum ὁ ἅγιος uno vel altero modo legendum est.

8 Exemplaris imaginem perscrutatus vocem φουρτ completam non esse, sed stelae dextrum marginem locum litteris supplendis praebere non posse vides, lector optime: itaque, si exemplar recte delineatum est, φο[υρτ] supplere non possumus, sed errore lapicidae assumpto aut φο(υρ)[τ] aut φο[ῠ](ρτ), quod veri similius esse videtur, emendandum nobis est.

1 Ι͡Σ

2 Χ͡Σ

3 ΟΑ...Σ

4 ΝΙΚΟ ΛΑ

5 ΟΣ

6 ΣΑΧΗ

7 ΡΗΦΟΥ[ΡΤ]

8 Χ.....

9 ...ΡΗΦ

10 ΟΥΡΤ

11 ΠΑΚΑ

12 ΘΑΡΠΑ

13 ΚΑΘΑΗ

14 ΦΟΥΡΤ

15 ΑΝΠΑ

16 ΛΑΝΑΠΑ

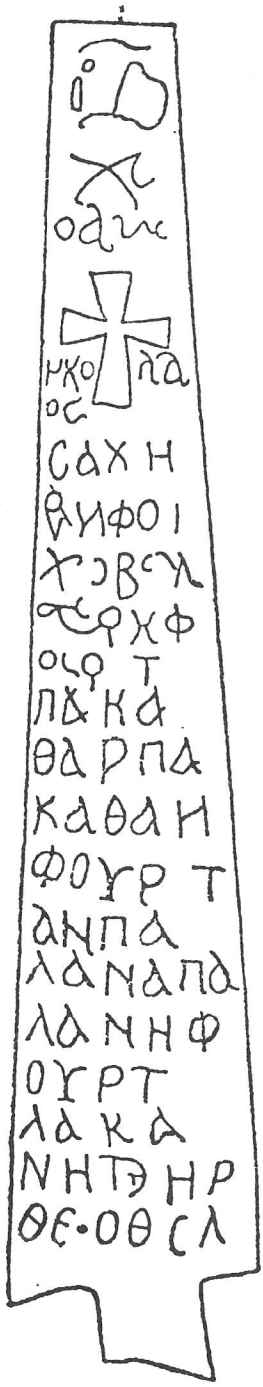
17 ΛΑΝΗΦ

18 ΟΥΡΤ

19 ΛΑΚΑ

20 ΝΗΤΖΗΡ

21 ΘΕ·ΟΘΣΛ



[9] (ρ) emendandum, non suppiendum est, exemplari nullum locum praebente.

[10] Secundum *a* delendum esse videtur: ⟨*a*⟩.

[11] Cf. Turčaninovii docti epistulam ad Basilium Joannis filium scriptam atque in commentario ad suam editionem (1971:267 sq.) ab eodem viro docto publici iuris factam.

[12] Has ultimae lineae quattuor litteras Turčaninovius doctissimus (1971:105) eodem modo in libro suo (de quo videas supra in notula 3) interpretatur, Rossici tamen textus, qui 'chronologicus' a recentioribus editoribus appellatur, brevem adumbrationem addens. Saeculo duodecimo Cyricus monachus in Rossicis litteris non ignotus (Rossice Kirik appellatus), monasterii Antoniani in Novgorod urbe diaconus et domesticus, de quae chronologia dicitur egit; quae pertractatio in virorum clarissimorum S. P. f. Obnorskii et S.G.f. Barchudarovii libro (1938:186 sqq.) doctiore publici iuris facta est. Monachus doctus certiores nos facit se anno 'post Adam' sexies millesimo sescentesimo quadragesimo quarto (6644) scribere, quod secundum alium Cyrici locum, in quo de revolutionibus solis agitur, ducentae triginta septem solis revolutiones totae et octo ducentesimae duodequadragesimae revolutionis anni sunt (*išьblo ot adama s' l' z' a poslědnęg kruga ideťь osmoe lěto*; lin. 117 sqq.). Ducenties tricies septies duodetriginta faciunt sex milia sescentos triginta sex annos (237 x 28 = 6636); additis octo ultimae revolutionis annis eundem numerum annorum, 6644, obtinemus. Haec Cyrici computatio totius interpretationis vel ratiocinationis Turčaninovianae solum fundamentum est. Attamen Cyricus, diaconus domesticusque doctus, non de annorum in rebus gestis indicandorum modo sed de aliis rebus partim astronomicis, partim astrologicis hermeticisque, partim ad religionem pertinentibus agit; exempli gratia dico *učenie imže vědati čiku čisla vsěch lět* (1.38: 'quo modo numerus omnium annorum statuendus sit'), *kako es(tь) razuměti čislu dnevnomu* (1.62: 'quo modo numerus diei intellegendus sit'), *učenie o i(n)diktě* (1.86: 'indictionis doctrina'), *kako možetь vědati sl͡nčny krug* (1.103: 'quo modo revolutio solis intellegenda sit'), *sice že razuměti možet krug lunny* (1.121: 'hoc modo revolutio lunae intellegenda est'), *o věcech mira* (1.139: 'de mundi aetatibus'), *o pronovlenii n͡bs* (1.143: 'de caelorum renovatione'), *na kolicě lětě obnovlęetsę more* (1.152: 'in quantis annis mare renovetur'), alia id genus. Computatio annorum per solis revolutiones, annorum turmas indictionis instar recurrentes, non ad res gestas applicatur; Cyricus ipse, monachus eruditus, cum de rebus gestis annalium modo agit, numerum annorum nec per solis nec per lunae nec per alias revolutiones indicat. Cf. lin. 1 sqq.: *Ot adama do potopa, v' s' m' i v' lěta. ot potopa do razměšėnia ęzykъ f' l'*, etc. 'Ab Adam usque ad diluvium duo milia ducenti quadraginta duo anni; a diluvio usque ad linguarum confusionem quingenti triginta, etc.'. Itaque Cyrici textus chronologicus dictus ad quattuor ultimas inscriptionis nostrae litteras interpretandas minime pertinere videtur.

[13] Etiam alias litterarum formas minus recte ad normam et minus constanter scriptas conferre potes: N in linea quarta; P in linea septima (cuius tamen quod hasta transversaria esse videtur solum exemplaris pro lapidis rima error plane est) a forma eiusdem litterae in linea quarta decima nec non duodevicesima valde differens; Z, quartam vicesimae lineae litteram; litterae H formas inconstantes in linea sexta, septima, nona.

[14] Cf. Gardthausen tabulam 7, columnam ad annum 1059 pertinentem.

[14a] Opusculo nostro iam perfecto Basilius Ioannis filius ab Aba prognatus per epistulam pridie Kal. Mart. anni aetate Christ. 1977 scriptam certiores nos fecit G.Th.f. Turčaninovium celeberrimum anno 1974 de inscriptione nostra disputantem lineas octavam et nonam Χοβς Ιστορ perlegisse eaque vocabula 'magnus Ows' interpretatum esse, quia *owsi* in lingua palaeo-Georgica Ossetarum nomen priscum fuerat. (Turčaninovii clarissimi disputatio numquam publici iuris facta est.) Vocem 'magnus' in secundo vocabulo bene recognovit Turčaninovius doctissimus; tamen Ηστορη non ad nomen praecedens pertinet, sed nominis sequentis Παϰαϑαρ patronymicum est. Si in linea octava cum Turčaninovio doctissimo nomen Οβς legere volumus, quid sibi prima eiusdem lineae littera X velit, nescimus. Quam litteram si T legere possemus, bene esset, quia ad vocem praecedentem pertinere posset: ΦΟΙ|Τ = φο[υ](ρ)|τ; quae lectio optima esset. Attamen haec littera (octavae lineae prima) solum X, non T esse potest, si lineamenta Strukoviana bona sunt; cf. X, tertiam sextae lineae litteram, et T quater in lineis decima, quarta decima, duodevicesima, vicesimaque bene scriptum.

[15] *-ii* latina genitivi terminatio est.

[16] Praesente opusculo iam scripto Iohanna Nichols, femina doctissima et rerum Caucasicarum peritissima, ad ea, quae Ia. S. f. Vagapov (1980:100–117) publici iuris fecerat, animum meum advertit. Lectiones alteras paene nullas Vagapov doctus praebet, sed titulum nostrum lingua Ingussica interpretari vult. Qua de interpretatione in altero loco loqui volumus; satis sint observationes tres. Primo: Vagapovii docti lectio *Sachiri fu(rto)j: Chob(a) SitIeri furt, Bagatar Bagatai furt, Anbalan(a) Abalani*

furt (i.e. Latine 'Sachir-ii filii: Choba SitIer-ii filius, Bagatar Bagata-ii filius, Anbalan(a) Abalan-ii filius') nullam interpretationem praebet: si Choba filius SitIeri, Bagatar filius Bagatai, Anbalana filius Abalani est, qui sunt Sachiri filii? Deinde: Quartam vicesimae lineae litteram epsilon esse (sicut vellet Vagapov doctus) haud est, qui credat: velit lector secundam vicesimae primae lineae litteram comparare, quae verum epsilon est. Postremo: Non est, qui credere possit, quattuor ultimas vicesimae primae lineae non solum sinistrorsum legendas esse, sed etiam propaenultimam earum pro Δ, paenultimam pro A fungi. – Opusculum hoc ante iam multos annos scripsi, sed propter fatorum invidiam Hoenigswaldii nostri doctissimi carissimique volumen gratulatorium non sine cunctatione ac mora publici iuris factum est. In quibus annis haud pauca ad nostram inscriptionem spectans materia collecta est: quam in opere *The Old Ossetic Inscription from the River Zelenčuk* ab Academia Vindobonensi edendo plenius tractabo.

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A KM Laryngeal as a conditioning factor for *s*-loss in Sora-Juray-Gorum

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1. The Austroasiatic phylum of which the Munda family is the westernmost[1] branch is notable for the complexity of its vowel systems. The Munda languages have less elaborate vowel systems than do their Mon-Khmer relations, but present numerous problems in reconstruction of their vocalism. One of these problems is the unexplained loss of *s* in Sora-Juray-Gorum in a small number of old core vocabulary items.

In this paper, we offer one explanation for this loss of *s* in otherwise 'inexplicable' circumstances in several Munda languages, notably in Sora-Juray-Gorum[2] and independently in Gorum.

S-loss[3] at the SJG stage is exemplified by a small set of cognate pairs showing ∅ in SJG and *s* in GRG (and elsewhere in Munda). Most of these forms have been known for some time (see Pinnow)[4]. Like a number of other sound changes in Munda that leave small, residual sets of problematic cognates (e.g., the forms with low tone in Korku), these forms presumably reflect a once general feature lost in most of the languages and preserved in a limited set of forms in one or two. We suggest that *s*-loss in SJG may be conditioned by creak (V̰) or creaky vowel – more precisely, *s* is lost before creaky voice. The creak hypothesis is new – nothing like it to account for *s*-loss in SJG has been suggested previously.

Table 1

	GRG	SJG	Other Munda	KM	English
1.	– –	*Vrɛŋ	Kharia *sɔrɛŋ*	*sVrɛ̰ŋ	'stone'
2.	*-si	SJG *ḭ, (h)ḭ, (h)iˀ SJ *iˀi/-i Go. *aŋid/-id*		*sḭ	'louse'[5]
3.	*sul-	*ol-		*so̰l-	'to crow'
4.	*soŋ-	*ɔŋ-		*sɔ̰ŋ-	'to sell, measure out'
5.	*-sob-	*ɔ̰- S. *ɔj-yɛr/-ɔ* J. *ɔj/ɔHyɛr* Go. *oh-yer*		*sɔ̰	'winnowing basket'[6]
6.	*sVŋɔl	*ʌŋʌl		*sVŋʌ̰l[7]	'firewood, fuel'
7.	*sVñ(l)ɔb	SJ *ʌne^{2}b, ʌnəb		*sVŋ(l)ə̰b	'tree, bush'
8.	*sVlu^{2}b	*ʌle^{2}b, ʌləb		*sVlə̰b	'chevrotain, antelope'
9.	*si(n)rɔŋ	Go. *in(=)raŋ*		*sinrʌ̰ŋ	'medicine'

	GRG	SJG	Other Munda	KM	English
10.	Re. *suti*ʔ ?GRG *sV(bk)tiC'	SJ *AtɬjH[8]		*sVt̲i̲	'female genitalia'
11.	sVbkleñ- Gu. *soleñ-* Re. *suleŋ-* Gt. *solæ-*	So. *uñul-da*		*sVlo̲/u̲ñ-	'to perspire'
12.	*sa( )l- Gt. *sa(-)boʔ* Re. *sa(-)bu*	*(Vn)ʌʔl/ʌHl So. *ɔnʌl* J. *ɔnalʌd* Go. *aʔal*	Kh. *sol* Ju. *esɔd* Mu. *sɛɛl*	*sa̲l	'mortar, husking hole'
13.	*sam- Re. *sam-*	Go. *am-sad-*		*sa̲m-[9]	'remove a pot from the fire'
14.	*sɛ2r GR *ser-	*ar- So. *argɔ-* "choke, suffocate" Go. *aH-raŋ-* "id." *arur-* "be hoarse" *aH-riŋ-* "choke on a bone"	NM *sVrɛXŋ-[10]	*sɛ̲r- [sær]	'have a hoarse throat, choke, sing, shout'

2. Creak in present-day Munda Languages

V̲ (creaky vowel) is found phonemically in Gorum, and phonetically in Juray.[11] No creak has been recorded for any of the other Munda languages nor, to our knowledge, for any other language of the Indian area. Insights into creak can be seen in the cognate phenomena (e.g., of Gorum creak with GRG). Some cognates of forms with creak in Gorum (and with reconstructed creak in SJG) have unexpected consonantal reflexes in GRG (e.g., Go. *toroH-*, SJG *tɔrɔ 'to snore', Gu. *torod-*, Ga. *torhwaʔ-*, GRG **tVr(h)ɔd-* (?); Go. *bioH-gi*, SJG *biyɔH; Gu. *bier* 'yesterday'. Again the number of relevant cognates is small. Other examples of Gorum *VH* (i.e., *V̲*) include: *teHmi*, SJG *teHme, tej2-me 'new', GRG *tVmɛ (Gt. *tmi*, Gu. *time*); Go. *jeHmol*, So. *jʌb-mɔl*, SJG **jeHmɔl*, jej2-mɔl[12] 'seed, paddy seed', GRG *cV( )mol(?), Gu. *suŋmol*, Re. *sumu*, Gt. *cmu*.

In Juray, creak seems to be primarily a function of the breaking of the vowel /ɛ/ into a diphthong [ɛ̲ᵊ]. However, there is a tiny residue of forms in which, phonetically, one gets [*V̲*] which cannot be attributed to this breaking. The forms are: *rɛ(H)(-)sa-sɬj* 'infant, newborn' (contrast *rɛ(-)saŋ* 'kind of neem tree used for hookworm'); *guHgud-* 'to scratch oneself' (cf. Gorum *guguH-* and Sora *ged-* with the same meanings); *tega(H)* 'friends and relatives'; and *(d)ri(H)yab-mʌd-* 'to blink'; also to be considered possibly is *siH/ʔ-siŋ-gɔl-* 'to whistle'.

While there are a number of other instances of creak (written here as *H*) in Juray, almost all other examples could be explained as a) either dissimilation of a consonant before an identical consonant, especially of *-d'* before *d-*, or b) as is probably the case even for *siH/ʔ-siŋ-gɔl-*, a kind of weakening (or fricativization?) of glottal stop.

A residue of forms is, however, not susceptible to this explanation. *guHgud-* might have been attributed to an ordinary reduplication of the form *gud-* 'to scratch' except that the cognate forms as well as the morphophonemics are suspect, and would indicate a more complex provenance.[13] The extremely small number of forms with *H* (*V*) and the fact that *H* only appears in one dialect of Juray, however consistently, rather than another (hence the parentheses around *H* in most cases) leads us to treat creak in Juray as a phonetic rather than a phonemic entity.

In Gorum, however, *H* is comparatively common, with both a wide enough distribution as well as essentially phonemically contrasting environments to warrant treating it as a phoneme. Cf., for example, *aHduŋ-* 'to screen, obstruct' and *a?doŋ-* 'to trouble, bother, pester'. *H* occurs in a variety of positions, except initially. Thus, one gets */aHsuŋ/*, CF */-suŋ* 'house'; */goHsaŋ/-* CF */-saŋ* 'cooked rice'; */oH-der-/* 'to winnow grain'; */denH/* 'contingent affix, affected'; */boHb-re/* 'sibling', */guguH-/* 'to scratch oneself' etc. Unlike the glottalized consonants, there seems to be no feature of release associated with it preceding vowels. *H* occurs primarily as marker of the [affected] function of the subject NP on the verb on which it is affixed. Creaky voice in Gorum, unlike the glottalization of obstruents, must be represented in underlying forms – there being no way to predict its occurrence or non-occurrence. Further, we do not get opposable sets of +/– creaky vowels; nor is *H* regularly derivable in any obvious way from glottal stop or from any other segments in the language, although some *H* are certainly derivable as a secondary weakening of glottal stop.

In fact, the nature of creaky voice in Gorum (and Juray for that matter) deserves some closer attention. There is a close relationship which obtains between the glottal stop and what may be tentatively labelled a laryngeal fricative.[14] If any simple characterization can be offered for it, the laryngeal (or creak) in Gorum is an intermittently voiced fricative homorganic with glottal stop. If (following Ladefoged and Catford) we understand creaky voice as a point on a continuum – i.e., in terms of a glottal parameter – then it appears to be essentially more akin to a 'feature' than to a segment. In Gorum, however, creaky voice is not just a 'bound' feature added onto other segments, or comprising a part of such segments (e.g. as is voicelessness, or release), but rather, its behavior indicates an independent segment rather like glottal stop. What the historical situation that the reflex 'creaky voice' represents is, however, a different question.

In any case, an investigation of creaky voice (in Juray) has been made using the spectrograph.

As in Gorum, [H] is utilized as the symbol for creaky voice, [V̰], in Juray; in one of the three idiolects under study it was the obligatory substitute for the glottal stop. It is found also (optionally) as a substitute for [yˀ] and sometimes [dˀ] before another consonant (especially if an obstruent) before morpheme boundary.

In spectrograms, creaky voice has a characteristic configuration and can be readily identified. It looks like a fricative, but shows up at lower frequencies than fricatives generally do; there is a slowdown of beats of the larynx; there seems to be less acoustic energy and this shows up in the spectrograms as interruptions or gaps in all the formants.[15] It loses frequency (i.e., presumably goes lower), and where it is the substitute for glottal stop /ˀ/ and appears between two like vowels (primarily in the idiolect of B. Boya), it shows a 10–15 millisecond gap between vowels, with each closure of the glottis showing up as a line of burst.

The spectrograms of Juray *H* thus confirm the acoustic phonetic impressions one has of creaky voice in both Gorum and Juray as a 'fricativization' of glottal stop, or an intermittently vibrated 'glottal stop'.

3. The Cognate Phonema of Creak in GRG

GRG reflexes of KM creak (as reconstructed from Gorum *H*, and SJG *s*-loss) have been identified in the consonantism, the vocalism, sometimes in both the consonantism and vocalism and in a few cases we find no trace of a reflex of creak at all.

GRG consonantal reflexes of creak can be grouped into those with non-glottalized consonantal reflexes and those with glottalized consonantal reflexes. The first set involves a continuant somewhere in the word (either a nasal or *r* or *l*). The second set includes the consonants *b, d, g (j), ʔ* and *h* (the first four are weakly voiced or devoiced pre-glottalized stops).

Let us consider the set involving some sort of glottalic or laryngeal involvement:

b: GRG *-sob, Go. *oH*, So. *ɔj/-ɔ*, SJG *ʂ̲ 'winnowing fan'; Gu. *tab(-)mi-*, Go. *taH(-)mu-*nuH 'sneeze'.

d: GRG *tVr(h)ɔd- (Gu. *torod-*, Gt. *torhwaʔ-*); Go. *toroH-*, So. *tɔrɔ-* 'snore'; GRG *gud- 'to comb, pull, sweep'; SJG *gud- but see note 13 regarding Go. *guguH-*, So. *ged-*, Ju. *guHgud-*.

g: Gu. *irig* CF-*rig*; Go. *ariH* 'millet, *Mandia* (Eleusine indica)'; GRG *bɬj-tir- with derived noun *b-in-iʔ-tir* and verb *big-tir* in Gutob, 'spittle, spit' (Re. *buitur*, Gt. *bweʔtur*) Go. *biʔ/j-tur-* 'spit' but *beH-rol-nuH* 'to spit up (of babies)'.[16]

j: cf. *big-/biʔ* above, and probably *irig*; GRG *g* after high front vowels is from earlier (pre-GRG ?) *j*.

ʔ: Gu. *aʔ(-)dur-*, Go. *aH-dur-* 'to jump across'. Note that Bhattacharya (for Plains Bonda (Remo)) has *suʔ* as well as *su-* for 'to crow' (GRG *sol-; possibly *sʂ̲l-). Another interesting but problematic form that is relevant here is the Gt. compound verb *waʔ-sar-* 'to call by shouting out loudly' made up of *wa-* (without glottal stop elsewhere) 'to call' (GRG **wai-*) and *sar-* 'to sing, sing out' (GRG *sɛ2r-), the cognate for the latter in SJG undergoing *s*-loss. We suggest that the *ʔ* in *waʔ* reflects creaky voice in the morpheme, *sɛ̲2r-, which follows.

h: The only KM language that has a fully phonemic *h* is Gtaʔ. Gt. *torhwaʔ-* (GRG *tVr(h)ɔd-) S. *tɔrɔ-*, Go. *toroH-* (SJG *tɔrɔH-) 'to snore'.

The second set, involving what can loosely be termed continuants, is examined next:

N: Gu. *taNdar*, So. *tadar*, Go. *taHdar, taʔdar* 'rooster, fowl': One could consider the Gutob form to have no reflex of creaky voice, but to have (as would similar forms without histories of creaky voice) 'spontaneous nasalization' before voiced stops. This is plausible enough, but for the other example of *N*, this explanation is not available. GRG *cVmol, SJG *jeHmɔl 'seed, paddy seed'. The Gutob is *sum̲mol* (Remo *sumu*, Gt. *cmu*). One would not expect a nasal before the *m* otherwise; creaky voice, we suggest, was reflected somehow, and the back segment or feature was (then) nasalized (in assimilation to the following *m* only in Gutob). Thus, GRG *cVmo̲l.

r: Gu. *bier*, Re. *er-og* 'tomorrow'; So. *biyɔji*, Go. *biyoHgi* 'tomorrow', J. *biyɔ* 'some day'. It looks as if the GR word for 'tomorrow' was composed of two pieces, the second being *-tog* (we find *-tog* elsewhere, as in Gu. *mar-tog* 'the day after tomorrow', *mun-tog* 'two days after tomorrow',etc. Thus the *r* may reflect not simply *V̲* but *V̲* plus a following *-t*).[17]

l: This example is problematic, and may involve a verbal morpheme with creaky voice and not the stem 'to come'. Gu. *olo* (defective imperative) 'come!', perhaps from *ol-a* (with *-a* the intransitive imperative marker), Go. *ɔH-ay* 'to come'. (Note too Korku suppletive *ol-en* 'went'.)

Vocalism: Our observations on vocalism are more tentative since the regular KM vowel correspondences have not been as solidly reconstructed as the consonant correspondences. The forms listed here are those where the GRG and SJG correspondences are notably deviant.

GR *wi-* (*ui-*), Gt. (Plains) *we-*, Gt. (River) *wai-*; Go. *uiH*(*-nuH*) 'to go'[18], GRG *tVrhɔd-, SJG *tɔrɔH- 'snore', and Gu. *bier*, Re. *er-og*, Go. *biyoHgi* etc. as mentioned above.[19]

Those forms showing no obvious traces of creaky voice in GRG are:

GRG **tVmɛ*, Go. *teHmi*, So. *təb-mɛ* 'new', (SJG *teH-me, SJ *tej^{2}-mɛ)[20], GRG *go- in Re. *go-lig-* 'to drape, wear a garment (of women)', and *go-si-* 'to drape/wear a garment (of men)', Go. *goHtuŋ* 'cloth, skirt', So. *gəd-tuŋ*, J. *gəd-tuŋ, gəH-tuŋ*.[21] GRG *bɔbreñ, bɔbrɛñ 'brother (female speaker)', So. *bəb-rɛ*, Go. *boHb-re* 'id.' GRG *-nə 'past participle marker', (SJG ??*bɔHb-rɛ) Go. *-nuH* 'infinitival marker, affected'.

With regard to *-nə/*-nuH and possible morphological affixal *H* morpheme(s) N. Zide noted (1960) that low tone/aspiration in Korku was significantly more frequent in verbal desinences, and nominal genitive and accusative-dative than one would expect merely by chance. Something similar has been observed for Gta? with respect to glottalized vowels (associated with weak stress) in verbal desinences and the genitive. It was also noted (N. Zide 1973) with respect to the reconstructed PM numerals that it appears as though one should reconstruct a 'morpheme of glottalization' for all or most of these. Are there, then, affixal morphemes represented (or substantially represented) by *H* ? Is *H* (also) a junctural marker indicating boundaries between, say, certain classes of stem and affix morphemes? Further, are there ablaut-like phenomena resulting from infixed creaky voice as a morpheme (or morphemes)? The data invite us to suspect, but not as yet demonstrate, all three.

4. The *S*-loss Hypothesis

Earlier hypotheses regarding *s*-loss had taken account of distinctive vocalism, accentual phenomena[22] and glottalization as conditioning factors. However, with limited success. Glottalization, i.e., both glottal stops and consonants, and N. Zide's 'glottalized vowels' did not solve the problem, although this seemed (and seems) more relevant than say, accentual phenomena.

The examination of creak in this context arose from a consideration of the possible phonetic properties of the conditioning factor. Creak, or creaky voice can be interpreted as a spirantal feature in view of the phonetic (including spectrographic) data as discussed earlier. One motivating condition for *s*-loss might be a constraint on multiple spirants, which results in the despirantization of the first spirant in a word.

The only spirant we reconstruct so far for Proto-Munda is *s.[23] There are examples in Korku (see below) of *s* de-aspirating before a spirantal *kh* (i.e., [x], e.g., as *sakhàn > *cakhàn* 'firewood'. In terms of the syllabic patterns of the Munda languages, consonant clusters that might spirantize to f, h etc. (e.g. Korku p becoming ϕ/_th in *apthàe* (from *ab-tha(-)e?) 'to wash the hands and feet',[24] or Go. p in *apay-u* becoming [afay] in allegro speech) are not found and we see no evidence for reconstructing them. In some KM languages, there is affrication of what are written [c] and [j] in several of the languages but are clearly (and reconstructed in the SJG) pre-palatal or palatal stops.

Since Gorum creak looks to be old (it is really odd, and presumably owes nothing to borrowing), we hypothesize that creak and *s* are the two spirants we reconstruct for KM (and for PM). We suggest that there is a constraint on spirants in SJG, that is, where *s* occurs initially followed by creak later in the morpheme, *s* is weakened and later lost.

4.1. Parallels in Korku:

Treating *s*-loss as a reduction of spirants invites consideration of parallels in the modern Munda languages. There are parallels to such a constraint in Korku, for example.

The development of a constraint on multiple spirants could provide some explanatory value to the ad hoc accounts given earlier (and the historical reconstruction based on them).[25] Taking creak as a spirant, and *s* (these being the only two KM (and PM?) spirants[26]) we can state a rule that where two spirants are found in a word the first is despirantized (i.e., *s* > *h*, and *h* later > ϕ). There are parallels in Korku where a sequence of two spirants in successive syllables is reduced to one – the medial one. (We alluded to this here above.)

There is some evidence of similar spirant reduction elsewhere in Munda. The two cases are: Korku *s . . s* > *c . . s*, and (?) *kh . . x* > *k . . x*. For the first case one might expect that an exception would be made with reduplicative formations – i.e., spirant reduction would not occur in reduplicatives. (Reduplication is common in the Munda languages, in nouns as well as verbs.) However, one of the instances of spirant reduction (by one interpretation) in the Ghatang dialect of Korku occurs precisely with reduplicated infinitives. In these, *sod* 'to fall, drop', for example, is not – as elsewhere in Korku (and similarly elsewhere in NM) – reduplicated ***so-sod*, but *co-sod*. Likewise for 'turmeric' *ca-saŋ* (elsewhere *sa-saŋ*). This, however, must be considered first as one instance of the application of a reduction of aspirates rule (the sort of rule common elsewhere, e.g., in IA, Greek, etc.) of the following kind: where a (voiceless) aspirate occurs initially in a monosyllabic verbstem, the initial aspirate of the reduplicated stem is deaspirated, e.g. *khad-* 'to enlarge', *ka-khad* [kaxàd]. The *c* in Ghatang Korku is functionally equivalent to an unaspirated *s* (as *k* is to *kh*). The *kh* before *a* and *o* (depending on the final consonant of the monosyllabic verbstem) tends to spirantize to *x*.

In the second case, a few morphemes of *sVkhV(C) (sVkàC) shape are found, e.g., NM *sakàn 'firewood', Ko. *sakàn* or *cakàn*, where the *s* despirantizes to *c* (in most, if not all Korku dialects). For instance, *cakhan* [caxàn] can be analyzed and transcribed *cakàn or *sakàn with the reduction of spirants rule. Kherwarian has the expected *sahàn*. One could suggest that the historical development of Kherwarian from North Munda also showed a reduction of spirants development, but one that worked the other way, i.e., the second, not the first spirant, was de-spirantized, to *h*, and in some dialects of Mundari further reduced (despirantized?) to ϕ, i.e., *saan*.

With regard to the *k* > *h* development in Kherwarian, conditioned (partly) by a glottalized vowel environment, the original basis for the *k* > *h* change in Kherwarian may have been a reduction (glottalization/aspiration ≅ spirantization) rule, where the *k* was spirantized in glottalic environments, and the (spirant) glottalization/aspiration conditioned the loss of the spirantized *k*. Thus, Kherwarian and presumably NM *jokaX > jokhaX > joxax > johaX[27] later reduced to *joha*, 'cheek'. (The Korku cognate *joka* has no low tone, and this would need explanation.) The glottalization itself was then lost in certain environments.

4.2. Creak in KM

The kinds of correspondences between Sora and Gorum, Juray and Gorum and between SJG and GRG seem, on the surface at least, miscellaneous, but in fact a clear set of features emerges from these various correspondence sets, namely:

1) correspondences which involve palatality;
2) correspondences with glottalic constriction of some sort (including periodic interrupted production of glottal 'stop');
3) some sort of retraction of the tongue-root;

and possibly,

4) 'spirantal' or fricative character.

These points need to be elaborated somewhat. For example, we get SJ *j[2]/Go. *H*, SJ *1/Go. *H*, SJ *r/Go. *H*, SJ *r/Go. *j*, SJ *1/Go. *j*, and even J. *rt*/Go. *Hṭ*. In each case, however different, palatality appears to be one feature.

In the case of glottalic constriction, we find some sort of glottal (e.g. a stop or *H*) corresponding to some glottalized consonant. Otherwise, we find glottalized consonants in correspondence where the point of occlusion does not correspond, as it does in the regular glottalized consonant correspondences. Or, as occurs in a number of cases in Gorum, while a 'regular' glottal stop, i.e., SJG *-g is associated with automatic nachklang of the vowel, in these cases, the glottal stop has no associated nachklang and ends the word as would an ordinary glottalized consonant, e.g., SJ *ʌsu but Go. *asu?* (i.e., not **asu?u), GR *Vsi, Gt. *a?si* 'ache, hurt, be ill'.

As noted earlier, the *H* (at least in the spectrographic materials we have from Juray) as well as our phonetic perception of it, is notable for its periodicity, showing up at lower frequencies than normal fricatives, but indicating a clear slowdown of beats of the larynx; there also seems to be acoustic energy which shows up in the spectrograms as interruptions or gaps in all the formants. We can consider this spirant-like, since the occurrence of 'noise' in the upper frequencies, the periodicity etc. are all reminiscent of acoustic descriptions of 'fricatives'.

Finally, the effects of 'creak' are not limited to a single segment; they affect at least one morpheme (word?), and may affect other words in the phonological phrase as well. All of this points to a register-like character. That is, we do not assume KM creak to be a separate 'phoneme', but rather we tend towards reconstructing a constellation of features which act on a word, or minimally on a morpheme, not unlike descriptions of 'register' in MK.[28]

4.3. Evidence of Palatality

There is evidence for palatality in the vowel correspondences: Sora /ʌ/ and Go. /e/. Deriving Gorum /H/ from *j is unconvincing for a number of reasons. First, as noted above, the Sora CF -ɔ instead of the expected **-ɔj presumes an evolution of perhaps *ɔH-yɛr/-ɔH where $H > (^{?}) > \phi$ finally, but assimilates to -y before palatals. Second, the vowel correspondences are all erratic; e.g., SJ *jʌb-mɔl/Go. *jeH-mol* makes no sense in view of forms like S. *jʌb-laŋ*, Go. *jab-laŋ-* ('stick out the tongue, etc.') so that it is clearly not the preceding palatal which produces the otherwise palatal environment required for correspondences of ʌ/*e*.

The comparative evidence from GRG and from MK suggests that there is no '*j*' before e.g., *m* in e.g., *jʌb-mɔl* : *jeHmol* and in fact there is nothing at all but an 'automatic' V (cf. e.g. Svantesson on Khmu' (1982:97)).

We cannot even, for SJG itself, suggest a reconstructed *j both because of the So. -ɔ, hence SJG *ɔH and the fact that /H/ in Go. is idiosyncratic in that environment, since *joj-mol (or jo'-mol) could conceivably have been the form for 'seed' but is not.

Similarly, with SJG *biyɔji/Go. *biyoHgi*, an original SJG */g/ > *j/-i in SJ, remaining /g/ in Gorum, so that the original form did not have a palatal in what is masquerading as a palatal environment.

Possibly Go. *H* corresponds to an SJ *j of some sort which one probably ought to distinguish from ordinary SJ */j/ with Gorum reflexes, hence our hypothetical *j2.

Once we go beyond SJG, there is no evidence of any 'consonant', palatal or otherwise. However, further indications of a palatality feature and found in other probably correlated 'palatal' consonant reflexes elsewhere, e.g., SJ *r : Go. *H*, SJ *ɾ : Go. *j*, etc.

Consider a) J. *tɔŋ-ar-* 'lead, dance, drag by the hand' : Go. *aH-ṭoŋ* 'id.' with simple SJ *tɔŋ-: Go. *toŋ-* 'lead' as well, where *ṭoŋ* with retroflex *ṭ* presumes *toŋ-* with a *ṭ* conditioned by the preceding *H* in Go. Retroflex *ṭ* in Gorum appears to be conditioned by the 'retraction' of the tongue or tongue-root in several other forms in Gorum.[29]

b) The *r* : *j* correspondences are relevant here too since if palatality seems to be intimately connected with the occurrence of *H* (SJ *j2) we cannot ignore the areal support for palatal *r* as well in Malayalam and Irula. There are *r* : *j* correspondences too, however few: witness, J. *lɛrpoŋ* : Go. *lej-puŋ* 'spare tire, rolls of fat at the waist' or S., J. *ler-* : Go. *lij-* 'spread, grow big, grow a new sprout (of root vegetables)' hint at a retracted, palatal of some sort. The recurrence of *r* : *j*, *r* : *H* as well as assimilated glottalized obstruents with *H* point to features of a) palatality and b) glottality.

GRG too, shows palatal consonants where Go. has *H*, e.g. the previously cited GRG *biɛr : SJG **biyɔH*, and GRG *sɛ2r 'to sing, shout out, raise the voice' : S., J. *ar-*, Go. *aH-go-* 'have a hoarse throat, etc.' and the Gu. *irig* pre-GRG *Vrij : Go. *ariH, arig, ari'* 'millet, probably Panicum'.

Going outside of SM, there are rare alternations of final *r* and *j* in e.g., Ko. *namur, namuj* 'to set (of the sun); to sink (drown)'. Note also the presumably doublet forms in Ko. *solod, solor* 'to slip', where the latter form has an alternate form with a high front vowel, *silir*.

4.4. Tongue-root Retraction

As to tongue(-root) retraction, as noted above, *H* in Go. falls into the same category of conditioning factors as *r, l,* (and even *g*, and *k*) for the rise of retroflexion of *t*.[30]

While the evidence is hardly overwhelming, it certainly bears consideration in determining the characterization of the possible phonetic properties of the reconstructed ancestor of creak. With reference to register, which has been discussed in detail[31], with regard to tongue-root position we would like to be able to identify 'chest' register in MK with NM low tone/aspiration (i.e. low register?) and perhaps KM V̲; and to claim that some of the characteristics Gregerson describes for MK chest register apply here.

According to Gregerson there seems to be a correlation between voiced and lax consonants on the one hand and voiceless and tense consonants on the other. He claims that advancement or retraction of the tongue-root can constitute a major airstream regulator.

We wish to claim here that whatever the ancestor of 'creaky voice' was, it was associated with the feature 'retraction of tongue-root'. It seems pertinent to note that many of the effects associated with such tongue-retraction, namely retroflexion, 'centralization' (and/or 'peripheralization') of vowels, lenition or weakening of articulation of associated consonants, etc. in a word are also associated with the effects of so-called 'chest register'.

Cognate reflexes, e.g. Go. /*H*/ : GRG *r at first seem dubious, but if we hypothesize something akin to chest register for KM with correlated tongue-root retraction this makes the correspondences of Go. *H* : (SJG V̲) with GRG *r, SJ *r and Go. *H* (and probably SJ *r and Go. *j*) more plausible. When the tongue-root is pulled back, the rest of the tongue must also be retracted. This necessarily includes the tongue blade and dorsal area as well.

4.5. Glottality

The third feature we would like to claim for the historical source of creak is one of glottalic stricture. Aside from the obvious reflexes of glottal stop, V̲, and h, in GRG there are reflexes with pre-glottalized b', d', and g' (this latter before high front vowels derived from pre-GRG *j).

Presumably, the bilabial closure in e.g. *tʌb-mɛ* is in assimilation to the following labial. It may owe something as well to the preceding vowel. Thus, the glottality is retained and secondary features of occlusion develop.

While as a first approximation one is tempted to describe the historical source of creak simply as creak or creaky voice (since these do not otherwise have a place in the reconstructed phonemic system) this is inadequate to account for the variety of vocalic and consonantal correspondences we have examined here.

There is a polarization and reduction of the three features of the proto-form so that in cognate pairs while one does not find the entire constellation of features together, one does find two of the three, usually palatality and glottality (*j* : *H*), or retraction and glottality (*r* : *H*), or retraction and palatality (*r* : *j*).

It is not surprising that a complex (and relatively unknown) feature-complex (?phoneme) should be simplified in a variety of ways.[32] However, unlike Proto-Indo-European laryngeals, the phonetic nature of which is still much in dispute, the phonetic feature-complex which we postulate here is eminently pronounceable and presumbably reflects a com-

paratively recent proto, whereas the Proto-Indo-European laryngeals are more ancient and more hypothetical.

4.6. Spirantization

Further, and crucial to our hypothesis regarding *s*-loss as a function of multiple spirant reduction, we would also suggest that a feature of 'spirantization' or 'fricativization' characterizes this entity as well. This feature-complex not only would account for the great variety of reflexes, but presents no problems in phonetic realization.

It becomes clear that what turns up as creak in certain of the modern languages, low tone and other vowel qualities in other languages, and a variety of glottalic consonants in several of the other languages needs to be examined in terms of its possible (i.e., pronounceable) properties in a reconstructed KM.

4.7. Summary

To sum up, we find a number of associated 'features' in most of the cognate sets which individually seemed unrelated to each other, but which a closer examination showed worthy of consideration as part of a single entity. Whatever produced creak, register, palatals, glottalized consonants and *h* can be shown to be a plausible single phonetic construct not unlike one or more of the hypothesized IE 'laryngeals'.[33]

That is, it is perfectly possible to produce a sound with the following features:

1. glottalic constriction,
2. tongue-root retraction,
3. palatality,
4. spirantization.

To our knowledge there is no comprehensive treatment of *s*-loss: where and how it took place in the numerous languages in which it has developed. If one tries to arrive inductively at a causal factor or factors for *s*-loss development in the various independent instances of loss, one finds no obvious common denominator, not even within a particular language family. A system with a single spirant seems to be common; in such a pre-*s*-loss system, in particular contexts and circumstances, the *s* may weaken its articulation, and reduce (usually) to *h*, which is then itself often lost.[34] In SJG other (e.g. accentual) explanations of *s*-loss are inadequate. We postulate the presence (in specified environments) of creak, and identify its phonemic properties, making use of the GRG forms cognate with those in Gorum to reconstruct these properties. We claim that *s* and creak are the members of the SJG spirant set, and hypothesize a process reducing multiple spirants to one. Thus SJG *s* in the (specified) environments where creak is present reduces to ϕ. Phonemic creak itself is then lost except in some environments in Gorum. This provides an account of one piece of the phonology of Koraput Munda which makes historical sense of the forms we find in the data, i.e. in the six contemporary daughter languages.

Notes

[1] Munda is spoken exclusively in India, over a large area. There are roughly 12 languages in the family. Greater Mon-Khmer (which may or may not be divided into two branches coordinate with Munda) is spoken throughout Southeast Asia from Malaysia through South China, and west into Assam.

[2] We use the following symbols and abbreviations throughout the paper:

SJG Proto-Sora-Juray-Gorum which is the proto reconstructed for Sora (So.), Juray (J.), and Gorum (Go.), three Koraput Munda languages spoken in Orissa and Andhra Pradesh.
GRG proto for Gutob-Remo-Gta?, also a Koraput Munda sub-group.
KM Koraput Munda is the proto for these six languages
Ko. North Munda language: Korku
Sa. North Munda language: Santali
Mu. North Munda language: Mundari
Ho North Munda language
Khw. Kherwarian is the proto for Sa., Mu. and Ho.
NM North Munda
PM Proto-Munda comprises NM, KM, Ko., and KJ
KJ Kharia-Juang
Kh. Kharia
Ju. Juang
Gu. Gutob
Re. Remo
Gt. Gta?

Other symbols used throughout the paper include:

V which represents an "automatic" (i.e. unspecified) vowel,
L which represents a continuant (e.g., *l, r, n, ŋ, m*), and
C' which represents a glottalized consonant (*b', d', g'*, or *j'*).

In the modern languages, Juray and Gorum, *creak* (*V*) is represented by *H*.

[3] We deal here only with *s*-loss in SJG. There are cases of *s*-loss in Gorum where SJG retains the KM **s*. On these see A. Zide (1982 and 1980). Another, perhaps related, change in SJG consonantism is KM (and PM) **t* > SJG **s* (probably before high front vowel(s)). See also Footnote 8.

[4] Pinnow's remarks on *s/ϕ* are, as usual, worth attention (Pinnow (1959), p. 244). Those of his cognates not listed and discussed here, in our opinion, are not cognate, or do not belong here.

[5] While the SJ cognation is quite clear, the Gorum form presents problems in that the final *-d* is unexplained as a cognate with SJ **?*. Although *?/d* correspondences are common in GRG this seems to be an isolated instance in SJG. However, it should be noted that a) various cross-correspondences between the glottalized consonants *-b', d', g', j'* with *?* in both directions are common in SJG; b) this "louse" word as *-id* occurs in Gorum in several forms, e.g. *anjid* "louse", but also *kui-id* "louse newly hatched from egg" and possibly *poŋ(-)id-* "to extinguish a lamp", which on the surface would not seem to be cognate, but the act of extinguishing involves pressing the flame (or wick) between the thumb and forefinger in a motion similar to that used for killing lice. Further, Gta? *po-si*, presumably from **poŋ-si-* means 'to crush lice between the thumbnail against the ball of the forefinger'.

It should be noted in this connection, that Munda cognation involves motions and actions rather than end-results. Cf. as another example, Go. *ir-da?-* 'to spin' with So. *gir-da?-* 'to slap the thigh' and Gu. *girag-* 'to spin'. (Spinning by hand is effected by slapping the fibers against the thigh in a rolling motion.)

[6] What seems noteworthy about the SJG correspondences is the fact that the Sora CF (the combining form of a noun attached to verbs) which one would expect to be a regular *-ɔj* is, in fact, *-ɔ* which would be possible only if the FF (full form of the noun) had had a glottal stop instead of a *j* finally. Since an hypothetical So. **ɔ?-yɛr* is also possible but is not in fact what we find, relegating it to a simple glottal stop is impossible.

The IA words for 'winnowing fan' (perhaps from IE as well, see Mayrhofer 1976:366) Hindi *sūp, supli*, Oriya *supā*, Skt. *śūrpa* (Turner, Item No. 12573, 1966:729) look something like GRG. **-sob*, with preglottalized *-'b*, but are presumably unrelated. (Kharia *supli* 'winnowing fan' (Bhattacharya 1983:104) is from Hindi *supli*.) A Dravidian etymology (Burrow, 1945:118) is *śo*.

[7] There is a question in disyllabic morphemes as to which vowel (or vowels) are to carry the creaky quality. First, can we say that the morpheme as a whole has creaky vowel quality, or that the vowels of disyllabic morphemes are independently capable of carrying creak? If the latter is the case, one would assume that the *s*-loss would be conditioned by an immediately following creaky vowel. It seems likely that creak is a property of morphemes (as is e.g., register).

In Gorum, which preserves creaky voice fairly commonly, it is frequently a morpheme itself (e.g. of affectedness in verbs and/or a junctural phenomenon). From the KM point of view, it seems plausible that creak could be a feature of morphemes in which case it could be written once on the final stressed vowel.

[8] SJ *ʌtɬj, Re. *suti?*, 'female genitalia', does not meet the CVL requirement. There is at least one other reason to find the cognation questionable: there is a remote possibility it could be a borrowing in Remo (from Desia *c/soṭli*) but this is by no means certain either. Also, its morphemic analysis is unclear. It is not certain whether it is one morpheme or two. If it is two, the second would be *-ti?*. If the cognation were incontrovertibly demonstrable this would require changes, e.g., of morphophonemic constraints on canonical forms conditioning historical rules, but would not thereby undermine the creak hypothesis.

[9] Contrast Go. *sam*, Re. *sam* 'speech, word, utterance', Gu. *samɔ*, Gt. *samwa* 'language'.

[10] NM *sVrɛXñ or *sVriXñ where X indicates low tone, aspiration, or (low) register.

[11] It looks as though there could be a residue of a formerly possible phonemic *H* even in Juray. Sora has no evidence of creak, however, phonetically or phonemically.

[12] The symbol j^2 is used to indicate correspondences between glottalized consonants in SJ which have assimilated to the following consonant (frequently but not exclusively to *-b'* before a following *-m-* initiated morpheme) but which display vowel correspondences preceding the glottalized consonant which are otherwise exclusively found in palatal environments. SJ *ə/Go. *e* correspondences are understood as SJG *e/-palatals. SJ *j^2 almost invariably corresponds to Go. *H*, i.e., does not correspond with Go. *j*. 'New' and 'paddy seed' above are good examples.

[13] There is an SJG *gud- 'to scratch'. As in most Munda languages, a CVC verb may be reduplicated to indicate a) repeated action and/or b) intensity of action, so that *gugud-* and *gudgud-* (R-*gud-*) are regular forms in e.g., Gorum where they mean either 'scratch over and over again' or 'scratch hard'. ***guHgud-*** in Gorum could be understood to come from a reduplicated *gud-* via *gu?gud* with a 'weakening' of glottal stop to *H*. However, the form we in fact find for 'to scratch oneself', a reflexive (i.e. not an intensive) is ***guguH-*** (not **guHgud-). The problems this presents are that a) there is in fact no *d* in the form to dissimilate from, the final 'consonant' is *H*, not *d*; b) elsewhere, *H* is a morpheme of affectedness of subject and the reflexive meaning certainly can be seen to come under such a semantic rubric; and c) the 'infinitive' form is ***guguH-nuH***, unusual in its permissible two glottalics within a word (the ordinary situation is for one of the glottalics to disappear); d) the correspondences for SJG are equally irregular: Ju. *gu gud-* is by no means an ordinary reduction of *d*. Further, the use of *-dʌm*, the regular and commonly found reflexive verbal morpheme is not evident. Sora *ged-*, as well, it totally unexpected. It has had a shift of vowel in a language otherwise not known for any instances of 'ablaut'.

What seems to be one explanation is that there is probably a reflexive (or affectedness?) infixed morpheme which has left decided traces on the internal makeup of the verb, and that either we have an instance of two distinct and separate verbs, one with the meaning 'to scratch', *gud- and a second verb with the meaning 'scratch oneself' ???*R--gəH, or R-gVH- (SJG *ə >S. *e*, J. *o*, Go. *u*). What seems far more plausible is that we have a verb *gud- 'to scratch' with an infixed morpheme of subject-affectedness (which exists in modern Gorum), and *H as a register-like morpheme on the word which affects a) vowel quality, hence So. *e*, and b) other glottalic consonants (e.g., *-d'*) within a word.

[14] This, in contradistinction to Wang's [ɦ] which he calls a voiced glottal fricative (1968:6) but which is really an approximant (cf. Ladefoged (1967:88ff.)). Gorum [H] is much more highly constricted and thus closer to actual glottal closure (or glottal stop) with which it, on occasion, in certain specifiable environments, alternates. Ladefoged (1964:16) describes laryngealized voice (Catford's 'creaky voice') in the following way'

> "In this state of the glottis there is a great deal of tension in the intrinsic laryngeal musculture, and the vocal cords no longer vibrate as a whole. The ligamental and carytenoid parts of

the vocal cords vibrate separately sometimes almost exactly 180° out of phase with one another, one end opening as the other is closing. This produces an apparent increase often an approximate doubling, of the rate of occurrence of glottal pulses. . . . Laryngealized voicing need not occur in implosive consonants, and equally it can occur without the downward movement of the larynx which must (by definition) be present in an implosive. We can therefore, separate out two kinds of glottalized consonants: implosives . . . and . . . laryngealized consonants (as in Hausa). . . . It is often not possible to make an absolute distinction between laryngealization and glottal closure; as in the case of so many other phonetic oppositions there is an infinite gradation between the poles of the two categories. . . ."

[15] Recordings and spectrograms were made of select sets of minimal pairs of vowels and of the glottalized consonants, glottal stop, and creaky voice in the speech of D. Gomango and B. Boya, Juray informants, at the Central Institute of Indian Languages, Mysore in February, 1980. Thanks are due to Dr. D.P. Pattanayak, Director for making available to A. Zide the excellent facilities of the CIIL. Thanks are also due to Nagarajarao, Jennifer Bayler, and B. Rajpurohit for their help with the technical work, especially for the preparation of tapes, and spectrographic materials and for the analysis of the spectrograms of the vowels and of creaky voice.

[16] The alternation of *ɸ* with *ʔ* in Remo also occurs for forms where glottalization was hypothesized, *gel, gɛXl- 'to make offerings, do *puja* to a god'. *sol also had been reconstructed with glottalization (X) by N. Zide (1965). What relationship obtains between glottalization and creak, and more particularly, how we can reconstruct *sol- and *gɛXl are relevant questions we cannot now answer.

[17] With regard to *bier* in Gutob, see Ramamurti's Gutob form *bar-tog* (Ramamurti, 1938), and N. Zide's comments (1978) made with no thought of *bier* and its relevance.

[18] In this connection, cf. too So. *yer-*, J. *jɨr-* 'to go' which become more plausible as possible cognates for Go. *uiH-* especially in view of the r/H correspondence sets we discuss later in the paper. (But note Gt. *wir* 'to run'.)

[19] Note that the *preceding* syllable (morpheme) in GRG *kV-sob (KM *sɔ) 'winnowing fan' exhibits strange vowel correspondences. Gu. *ki-sob*, Re. *ke-sub*, Gt. (Plains) *ki-soʔ*, Gt. (River) *kẽ-soʔ*.

[20] For GRG *tVmɛ and *cVmol have a regular automatic vowel in the first syllable, which has creaky voice as has the whole morpheme. The vocalism in SJG is something else. It is not clear whether the GRG automatic vowel is to be reconstructed for KM, or something closer to the SJG *VH.

[21] Here too the vocalism, i.e., the correspondences between *ə/oH* and *ə/eH* are unusual, the normal correspondences between SJ ə and Gorum being *ə/a*.

[22] Cf. A. Zide, *S-loss in Sora-Gorum* (in press).

[23] How one treats glottalization of vowels is not unrelated to this discussion and something will be said about this further on. /h/ is probably to be reconstructed in certain expressive functions, but not as a regular phoneme of any widespread distribution. *h* does occur, but as a reflex of PM *k in Kherwarian and in Gtaʔ and possibly in pre-Gorum in restricted environments. One does find [x] in Juray and Korku, but they are clearly instances of restricted allophones of *k* and *kh* respectively in true medial position. There is no question of their being independent phonemes.

[24] Note too Go. *aH-si-nuH* 'wash the hands', *aH-jiŋ-nuH* 'wash the feet' and *aH-mad-nuH* 'wash the face' in connection with Ko. *ab-*.

[25] The situation in the Indo-European languages (much of this we owe to Eric Hamp, personal communication, 1983) seems quite different, and suggests no solution to the Munda *s*-loss problem. In IE, for instance, there is no vowel breaking associated with *s*-loss (in the IE families which undergo *s*-loss), nor any multiple spirant conditioning. There is, however, in both PIE and PM a similar pre-*s*-loss 'system', i.e. one with a single spirant, and laryngeal features (for Munda one, or possible two, we will not involve ourselves in controversies about how many, which, etc. IE laryngeals there are). And these 'laryngeals' are similar in both families in that they account for odd vowel correspondences (affect vowel coloration, and raising and lowering), aspiration and glottalization. In Gorum there is something paralleling the development from Iranian -VsV- to Avestan -*ŋh*- ([ñ]?). In Gorum, in a small number of forms, A. Zide reconstructs -h- for pre-Gorum in order to account for both morpheme-initial earlier k and s which show up in Gorum as -*ŋ*-. Thus, Go. *aŋon*, 'son, child', presumably from pre-Go. *Vn-hon, So. *ɔʔɔn*, Ju. *a-ɔn*, NM *kɔn, Khw. *hɔn; Gorum *aŋid/-id*, but SJ *iʔi/-i*. The Gorum form is presumably from pre-Go. *Vn-siH(?). We find in Gtaʔ *h* from earlier *ŋ*, thus GRG *tVŋɔn, Gt. *thwã* 'to stand'. (The SJG form metathesises the *n* and the *ŋ*, i.e. SJG *tVnʌŋ.)

[26] In contrast to voiceless aspirates, voiced aspirates in parallel contexts (e.g. *ghal-* 'to show') behave differently: by a general rule in Korku no voiced aspirates occur non-initially, and the reduplicated form of *ghal-* would be *gha-gàl*, the aspiration of the second *g* being absent, but the low tone (of the low tone/aspiration phoneme) being maintained. The low tone automatically occurs with medial aspiration, thus *kakhad* above is phonetically [kaxàd]. See N. Zide (1960) for further discussion of interpretations of 'low tone/aspiration' in Korku.

[27] Here X is meant to represent glottalization.

[28] Treatment in terms of prosodies, long-components, etc. could be provided. For descriptions of register in MK see e.g., Gregerson (1976) and the references he cites.

[29] Since a preceding /r/ exists as one of the conditioning factors for the rise of /ṭ/ in Gorum, this makes it unlikely that J. *ar-*, Go. *aH-* are to be rejected as distinct affixes.

[30] Note that Gta? (but not GR) also has a retroflex *ṭ* the origins of which are obscure, but that retroflexion seems to be correlated with "glottalized" vowels and perhaps with pre-glottalized *b* in particular. Note too GR *bVtoŋ- "be afraid", Plains Gt. *bṭo*, River Gt. *bṭʜŋ* (SJG bVtɔŋ-). The *o in GR was interpreted (N. Zide, 1965) as a glottalized vowel.

[31] See Gregerson, 1976, and Shorto, 1966 where, in reference to Mon, he says,

> "The paratonal register distinction is broadly similar to that described for Cambodian by Henderson. Its exponents are distributed throughout the articulatory complex but exclude pitch features. Chest register, symbolized by a grave accent placed over the vowel (ket, hakoa) is characterized by breathy voice quality in association with a general laxness of the speech organs and a relatively centralized articulation of vowels. The more frequent head register is unmarked in the transcription (ket, hakoa); it is characterized by a clear voice quality, relative tenseness, and peripheral vowel articulation."

[32] Note the fate, for instance, of Indo-European labiovelars in the various Indo-European languages.

[33] The Munda features in any case are much closer to the spoken languages than are the IE constructs. One wonders if anyone anywhere has suggested that PIE may be like some of the MK languages, i.e., a 'register' language.

[34] Instances of such loss of *s* via *h* are evident in Caribbean Spanish and Andalusian Spanish. In Puerto Rican Spanish the change from $h > \phi$ has taken place in the last twenty years. Cf. also note 25 above. The $s > h$ change is a common one. In the Indian area we have cases from Rajasthani dialects, Bhili dialects, Bengali dialects, Assamese dialects and Sinhalese (Sinhala) as well as from Central Dravidian. Only the last might be relevant to a (partial) historical explanation of the SJG sound change. Note that the Koraput area (and neighboring regions to the south) lacks aspiration: the Indo-Aryan Desia ('Kotia Oriya') lacks it, as do five of the six KM languages (the exception is Gta? where *h* reflects SM *k as well as SM 'expressive' *h*), and the Dravidian languages of the area, including non-standard Telugu.

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OIr. *·tab(a)ir* 'brings', *·taít* 'comes'

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1. The first vowel of *·tab(a)ir*, verbal noun *taba(i)rt*, has been the subject of notice by careful accounts of Old Irish; see *GOI* 53, § 82. It has been supposed that the *a* spread from the perfect *·tarat* 'has given' < *to·r(o)-ad-d-* (cf. *Celtica* xi, 74) < (*to*+)*ro-ad-dă-* < **pro-(ad-)dŏ-* < deH_o- 'gave' (cf. *Studia Celtica* xiv-xv, 1979–80, 107).[1] A similar spread could equally be justified, but on slightly different semantic/syntactic grounds, by starting from **pro-ad-dhə-* < **ad-dhH$_e$-*. In this case I would equate **ad-dhə-* with Phrygian αδδακετ, and explicitly because I consider Phrygian genetically a Western IE language; I refer to the well known clause exemplified by ως τα μανκαι κακουν αδδακετ (τι) and ως νι σεμον κνουμανει κακον α[δ]δακετ. It is possible naturally to link the sense 'puts, places, sets' (*DIL* D [1959] 208–10) with a reconstruction **pro-ad-dhə-*, but it must also be granted that semantically this sense could readily be derived from deH_o-. Yet we shall see that there is considerable argument for both these pre-forms.

It has been pointed out (*GOI* § 82) that the archaic pret. *·tubert ·tubart* does not show *a*, with the implication that the spread from *·tarat* had not then taken place. But at least one of these (*contubart* Thes. II 242,20 = Lib. Ardm., even though **e* is not preserved here) clearly means 'gave'. This form therefore does not, strictly, tell us anything about the prehistory of the value *·tab(a)ir* 'brings'.

It may be suggestive of the background of *·tab(a)ir* 'brings' to recall that in Later Phrygian αββερετ and αββερετορ were apparently functional equivalents of αδδακετ and αδδακετορ. Now we know that the pre-form of *tabhair* cannot have been simply **ad-bhere* > **(t(o)+) abbere*; the phonetics will of course not support this. Moreover, I have shown (*ZCP* 39, 1982, 209 and 212–14) that **ad-bher-* in Celtic meant 'offer'. Nevertheless, the vocalism of **t(o)-a-ber-* suggests that at an earlier time the *to*, which we know not to have been originally a preverb, may have taken the place of an older moribund preverb. The further thought suggests itself that a transformed match for **t(o)-a-ber-* → **t(o)-ad-b̥r-* may be found in the suppletive Welsh imperative *dabre* 'come!'; cf. Lat. *te adfers, urbem adferimur.*

I therefore suggest the equations
OIr. *·tarat* < **pro-(ad-)dŏ-* = Welsh *rhoddi* = Latin *prŏdŏ* = προδίδωμι
OIr. *·tarat* < **(pro)-ad-dhə-* = Phryg. αδδακετ = Latin *addŏ*
Then the aoristic **ad-dheH$_e$-* would have found itself in a natural suppletion with a compound of the presential **bher-*.[2] Thus we may equate
OIr. *·tab(a)ir* : Welsh *dabre* : Phryg. αββερετ : Lat. *adferŏ.*

Underneath this last equation I propose that there was an old compound (i.e. complex verb, or collocation) antedating the productivity of the Western IE **ad*, with a particle

TOWARDS ending in a syllabic and which could have produced Celtic *[a]. We find the best match for this in Iranian: Sogdian *"βr-* 'bring', *"β'yr* 3sg. impf. < ***ā-brya-*** 'was brought', suppletive to *"γt-* < ***ā-gata-*** 'come (used as a transitive)' (I. Gershevitch, *A Grammar of Manichean Sogdian*, 1954, §§ 540, 603, 653).[3] This selfsame suppletion is continued in modern Yaghnobi *ūr- ~uxt-*. The compound occurs with clarity in Avestan *ā-bar-* and in the Pamir dialects e.g. Yidgha – Munji *āvər-* 'bring'.[4] The exact vocalic background of this Indo-Iranian *ā* is agreed to be obscure. If in origin the particle was ***(H)eH***, a zero-grade would have produced ***ə* > Celtic ***a*. Such a slender body would account for its early demise.

We recover, then, ***pro* + *deH*$_{o}$-** 'give away', ***(H)eH* + *dheH*$_{e}$-** (> ***ad* + *dheH*$_{e}$**) 'place somewhere', and ***(H)eH* + *bher-*** 'bring'. Apart from the usual appeal to (morpho)phonology, syntax and semantics, we reconstruct these preverb collocations by exploiting criteria of morphological suppletion and Indo-European dialectology.

2. Indo-European had an old pattern of expressing the notion 'come' by a collocation *TOWARDS + GO*. Thus Polish *iść* 'go': *przyjść, dojść* 'come'; Serbo-Croatian *ìći ȉdēm* 'go', *otȉći òtīdēm* or *ȍdēm* 'go away', *dóći dōđēm* 'come'. Similarly, Old Irish matched *téit* 'goes', *tíagu* 'I go', subj. *téis*, fut. *'rega, lotar* 'they went', perf. *do'coïd 'dechuid* etc. with *do'tét* 'comes', *do'tíag* 'I come', subj. *do'tías*, fut. *do'rega, do'lotar* 'they came', perf. *do'dechud* etc. Notice that we appear to have *do* ← *to* + *GO*. The prototonic forms of 'come', however, show *'taít* 'comes', *'taíg 'táeg* 'I come', subj. *'táes*, fut. *'terga 'tirga, 'tultatar 'tullatár* 'they came', perf. *'tuidched* etc.

The consonantism of prototonic 'come' is explained by a rule (*GOI* § 179) applying to sequences C$_1$ VC$_1$ . . . , but Thurneysen is obliged to reconstruct (schematically) ***ta-thet-*** etc. < ***to-***. The *a* is attributed, improbably, to the impv. 2sg. *tair*, which in turn is supposed (*GOI* § 588) to be from *t-air-ic* somewhat anomalously. For *'tair* as a shortened form of the 3sg. pres. subjunctive of *do'airic* 'comes' see V. Hull, *Lg.* 25, 1949, 132. If we can explain *'taít*, I propose that likewise *tair* 'come!' is to be given a simple relation to *eirg(g)* 'go!', ***t(o)-a-erge***. Note that the outcome calls for *['a], and cf. below.

Observe now that *'ta-* appears only in the presential system, largely ***(s)teigh-***, of 'come'. This agrees well with *'tab(a)ir*, and we know that semantically *BRING/TAKE ~SEND* may be viewed as factitives of *COME/GO*.[5] Moreover, the perfect ***dī* + *ko-u̯ed-* > *'dechuid*** includes preverbs which could have rejected an old ***-a-***.

Analogously to ***(H)eH* + *bher-***, therefore, I compare OIr. *'taít* 'comes' to Skt. ***ā* + *gam-***; *'táeg* 'I come' is therefore ***t(o)-a-tēgū***, with ***t(o)-'a-***.[6] That is, if SEND = GO-FACTIT and COME = TOWARDS + GO, then BRING must be COME-FACIT = TOWARDS + GO-FACTIT. Thus we may quite simply derive

TOWARDS + GO → *(to)-a-tēg-*;
TOWARDS + GO-FACTIT →
(to)-a-ber-

It is expectable, then, that the suppletion in these two verbs should be homomorphous or coextensive; for at bottom it was identical. And the ancient preverb may be said to have participated here in but a single abstract collocation. The fundamental correspondence is therefore that with Indo-Iranian ***ā* + *gam-*** (and the suppletions of ***g^{w}em-***). In view of the presumption of the presence of ***(H)eH* > *a-*** and of its insubstantial moribund status, I

submit that we may now understand how the stem of Skt. *gam-* 'go, move, depart' came to mean 'come' in Latin *ueniō* and the Germanic etymon which *come* represents. That is, the simple base of IE **g^{w}em-* 'go' appears in Western IE dialects in the meaning 'come' by virtue of a loss of the once semantically essential and distinctive **(H)$\underset{o}{H}$ > *a* probably through sandhi phonetics or ambiguity. In Italic and Germanic this slender particle/preverb was not protected by a sentence connective as we see that it was in Celtic.

Notes

1 The most direct derivation of this compound is surely from **deH$_{o}$-*, *pace GOI* 35, § 50 (note), whose paradigm would have included **dH$_{o}$-* = *də-*, which would of course have been homophonous with **dhH$_{e}$-* = *dhə-* in Celtic. This is justified by the pervasive semantics (see *DIL* D [1959] 204–8) as well as by its syntactic behaviour and the separate syntax of **kor- ker-d(h)-* (*'cu(i)rethar fo'ceird*; cf. *ÉC* 18, 1981, 113 and *MSS* 40, 1981,43–4), which seems to be the main continuant of IE **dheH$_{e}$-*.

2 On the suppletive nature of **bher-*, see *ZCP* 39, 1982, 205–18. Remember that **pro* could be simply perfective in Celtic.

3 Further on the idiosyncracies of 'come', 'bring', 'give' see my paper on Welsh *moes* 'give me', Poznań Conference on Historical Linguistics, 1983, which will be incorporated in a separate publication.

4 A similar match is found in turn for **ad-dheH$_{e}$* in Skt. *ā + dhā-* 'place, set, lay, put'. Note the entirely different semantic relation observed in the directionality of Skt. *ā + dā-* 'take, receive, deprive'. Yet *pra + dā-* 'give up, deliver, bestow' exactly corresponds to Welsh *rhoddi* = Lat. *prōdō*. These Indic facts precisely confirm the analyses **(pro)-ad-dhə-* and **pro-(ad-)dō-* above.

5 Cf. the Sogdian and Yaghnobi above; and Robert Binnick, "On the nature of the 'lexical item'," *Papers from the Fourth Regional Meeting*, Chicago Linguistic Society 1968, 1–13.

6 I elaborate this accentuation with **to*, and its implications, elsewhere.